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People say that a PhD is a marathon, not a sprint. Over the past five and a half years, I have gained experience with both: long-distance running and writing a PhD thesis. I have personally found the latter even more challenging than the former for several reasons. When you start a PhD, you don't even know the route. There are detours and dead ends, and unlike a marathon where the finish line is clearly marked, the end is often uncertain. Most importantly, while you can run a marathon alone, I would never have been able to complete this thesis without the support of many great people.

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Contents

List of Figures

List of Tables

Introduction

Credit is a fundamental pillar of modern economies. Households and firms rely on credit to bridge the gap between current financing needs and future income. Financial intermediaries, particularly banks, facilitate this process by channeling funds from savers to borrowers. Credit thus enables investment and economic growth but also introduces risks. Accelerated credit expansion, particularly through bank lending, has been identified as a key predictor of financial crises (Mian and Sufi, 2009; Schularick and Taylor, 2012). At the same time, well-functioning credit markets are essential for economic development and welfare (Beck, Levine, and Loayza, 2000; Levine, Loayza, and Beck, 2000). Given the critical role and inherent risks of credit, it is not surprising that public institutions actively engage in shaping credit dynamics. Therefore, understanding the forces that drive credit supply and the mechanisms through which public institutions intervene is of first-order importance.

This thesis contributes to the understanding of credit markets by analyzing the role of financial institutions and policies by public institutions—specifically, monetary policy, government-backed credit programs, and macroprudential regulation—in shaping credit outcomes. It consists of three self-contained essays that investigate different aspects of credit markets: (i) the interaction between monetary policy and evergreening behavior in the corporate loan market, (ii) the externalities of credit supply expansion in mortgage markets on housing markets and lending behavior, and (iii) the effects of monetary and macroprudential policy on bank lending rates.

The first chapter documents that monetary tightening induces evergreening behavior in the corporate loan market. Using granular loan-level data for all euro area countries, I document that following contractionary monetary policy shocks, a bank intuitively cuts credit supply, including to firms that have maturing loans outstanding with that bank. However, this reduction in credit supply is smaller when such firms with rollover needs are closer to default. This allows these firms to roll over more maturing loans at lower interest rates. I argue behave in such a way because they have incentives to "evergreen" loans (Faria-e-Castro, Paul, and Sánchez, 2024), that is, to increase credit supply to existing borrowers that are closer to default - a strategy aimed at preventing these firms from defaulting, thereby allowing banks to avoid recognizing losses on their outstanding loans. To identify a credit supply channel, I exploit the fact that different banks estimate different default probabilities for the same firm in the same quarter due to idiosyncratic differences in credit risk models.

To assess the importance of the evergreening effect, I conduct a back-of-the-envelope calculation, which shows that banks' evergreening behavior reduces the monetary policy-induced financing gap by up to 30% for a firm close to default. Moreover, the effect is asymmetric and much stronger after contractionary than expansionary shocks. I provide further support by using confidential data on bank capitalization to show that the effect is stronger for banks that would benefit more from issuing a rollover loan to a risky firm to avoid its default. These findings complete the risk-taking channel of monetary policy (Ioannidou, Ongena, and Peydró, 2014; Jiménez, Ongena, Peydró, and Saurina, 2014; Dell'Ariccia, Laeven, and Suarez, 2017; Paligorova and Santos, 2017), which documents a more pronounced credit supply response for riskier firms in the absence of existing loans to the bank. Thus, my findings establish a novel mechanism for understanding the interaction between monetary policy and borrower risk. I show how monetary tightening leads to relatively more credit supply to risky firms when they have rollover needs from outstanding loans to the bank. Overall, this implies a weaker transmission of monetary tightening to risky firms with rollover needs at the expense of potential long-term risks to financial stability.

The second chapter (joint work with Moritz Kuhn and Farzad Saidi) provides long-run evidence on credit externalities and the housing market. While existing research highlights the role of credit supply in driving house price fluctuations, empirical identification is often complicated by the challenge of disentangling credit supply from shifts in house price expectations. To address this issue, we leverage a novel dataset covering mortgages guaranteed under the U.S. Veterans Administration (VA) loan program since the 1980s. We exploit a quasi-experimental expansion of the program's eligibility criteria following the Gulf War, which led to a localized and exogenous increase in credit supply. By studying the long-run effects of this credit expansion, we document a sustained increase in house prices in regions with a higher concentration of newly eligible veterans. Moreover, we find evidence of a spillover effect: house price growth induced by a credit supply expansion in the VA segment of the mortgage market incentives lenders in the conventional mortgage market to expand credit supply, thereby amplifying the initial impact of the credit expansion. This feedback loop—where credit-induced house price growth fuels further credit expansion—sheds new light on the mechanisms through which credit conditions shape housing market dynamics.

To establish causality, we employ a Bartik-style identification strategy that interacts a county's pre-determined exposure to veterans, measured as the distance to the nearest military base from which soldiers were deployed to the Gulf War, with national variation in VA loan take-up rates over time. We find that a one-standard-deviation increase in VA credit supply raises house prices by about 6% in the following year, with effects being further amplified for up to five years. The response is stronger in regions with inelastic housing supply, underscoring the role of supply constraints in amplifying credit-induced house price fluctuations. Furthermore, we document that lenders in the conventional loan market respond to rising house prices by easing credit conditions, leading to higher mortgage approval rates and lower interest rates. This suggests that beliefs about future house price appreciation are key in reinforcing credit cycles. Overall, our findings highlight an important externality of mortgage credit: by altering expectations and influencing lending behavior beyond the directly affected segment, credit supply shocks can have far-reaching implications for housing markets and financial stability.

The third chapter (joint work with Jan-Hannes Lang and Marek Rusnák) examines the relative importance of monetary and macroprudential policies for corporate lending. Using a granular dataset of corporate loans in the euro area combined with bank supervisory data, we find that monetary policy has a substantially larger impact on lending rates than changes in macroprudential capital buffer requirements. In our empirical analyses, we use a bank's capital-to-asset ratio to assess the impact of macroprudential policy. We find that even the smallest possible change in monetary policy, 25 basis points, would require an increase in macroprudential buffer requirements of about 2.4 percentage points to have the same effect on bank lending rates, which would be a large change in the macroprudential stance. These results are consistent with a simple theoretical framework of bank lending rates that guides our empirical analysis.

However, the relative dominance of monetary policy decreases - but persists - under three conditions. First, as policy rates approach the zero lower bound, monetary policy transmission weakens, and the impact of bank capital increases, reducing the relative dominance of monetary policy. Second, in financial systems where the corporate bond market is less developed and there is thus less competition for corporate lending, banks' ability to pass through changes in funding costs is higher, which also reduces the relative dominance of monetary policy. Third, when banks have higher capital levels, this dampens monetary policy transmission. We use a set of fixed effects and time-varying control variables to isolate credit supply responses. Furthermore, we provide robustness by using high-frequency monetary policy surprises as an instrument for the monetary policy stance. Additionally, we show that differences in loan terms do not drive our results. The findings highlight the dominant role of monetary policy in shaping corporate borrowing costs while underscoring the conditions under which macroprudential measures become more influential.

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Chapter 1

In for a Penny, in for a Pound? Legacy Debt and Lenders' Response to Monetary Tightening

1.1 Introduction

The recent episode of monetary tightening prompted a critical question: Do banks help vulnerable firms get through such difficult times, or do they contract credit just when firms need it most to survive? Monetary policy shapes banks' credit supply and, consequently, the terms on which firms obtain new loans. Firms seeking financing for new investment may postpone projects if credit conditions deteriorate, but firms that need to roll over maturing debt are particularly exposed to deteriorating credit conditions. Faced with tighter conditions, these firms must choose between liquidating valuable assets to repay maturing loans or accepting higher future interest expenses on rollover loans. As central banks tightened monetary policy in 2022, concerns emerged that adverse credit conditions would disproportionately affect firms with significant rollover needs - especially those that are already riskier ex-ante.[1] However, this view assumes a uniform bank response to all firms with rollover needs, regardless of how a reduction in credit supply would affect a firm's risk of defaulting on its outstanding loans with the bank. In fact, Faria-e-Castro, Paul, and Sánchez (2024) document that banks selectively adjust credit supply to firms with loans already outstanding to them. In particular, they have incentives to "evergreen" loans, i.e., to increase the credit supply to existing borrowers that are closer to default - a strategy aimed at preventing these firms from defaulting, thereby allowing banks to avoid recognizing losses on their outstanding loans. If

[*] Any views expressed are only those of the author and do not necessarily represent the views of the ECB, Deutsche Bundesbank, or the Eurosystem.

1. This concern was raised by central banks (e.g., European Central Bank, 2024; Kadyrzhanova, Perez-Orive, and Singer, 2024) and journalists (e.g., Howell, 2024).

evergreening is particularly pronounced during periods of monetary tightening, it may allow vulnerable firms to weather such periods. This paper, therefore, studies how banks' evergreening behavior interacts with monetary policy.

I empirically examine how bank debt of firms with rollover needs evolves in response to changes in monetary policy and how this depends on firms' ex-ante default risk. I leverage granular supervisory data and employ fixed effects regressions to isolate the differential impact of default risk on credit supply. I find that banks' responses are moderated by borrower risk: credit supply adjustments to monetary policy are smaller for firms closer to default. The differential response is substantially stronger following contractionary than expansionary monetary policy shocks. This shows that monetary tightening induces evergreening behavior: banks cut credit supply to firms closer to default less after monetary tightening to reduce the likelihood of incurring losses from defaults on outstanding loans. I provide further support by using confidential data on banks' capitalization to show that the effect is stronger for banks that would benefit more from issuing a rollover loan to a risky firm to avoid default. These findings complete the risk-taking channel of monetary policy (Ioannidou, Ongena, and Peydró, 2014; Jiménez, Ongena, Peydró, and Saurina, 2014; Dell'Ariccia, Laeven, and Suarez, 2017; Paligorova and Santos, 2017), which documents a more pronounced credit supply response for riskier firms in the absence of existing loans to the bank.[2] My findings thus establish a novel mechanism for understanding the interaction between monetary policy and borrower risk. I show how monetary tightening leads to relatively more credit supply to risky firms when they have rollover needs from loans outstanding with the bank.

My analysis is based on a link between the bank lending channel and lenders' evergreening behavior toward borrowers with existing debt. Under tighter monetary policy, banks typically reduce credit supply as financing costs rise, which creates a financing gap for firms seeking to roll over maturing loans. This gap is particularly severe for firms closer to default, as adverse loan terms can push them into insolvency. This would trigger losses for banks on existing exposures. To avoid such outcomes, banks with legacy exposures towards a firm have an incentive to moderate their credit cuts to riskier borrowers by extending more favorable loan terms. This selective adjustment of credit supply is likely to be more pronounced in periods of monetary tightening than in periods of monetary easing. In particular, banks with greater ex-ante capital headroom should be less likely to reduce credit supply

2. While the results on the risk-taking channel are mostly interpreted in the context of loose monetary policy, the empirical studies do not distinguish sign-dependent effects and would, therefore, also support the conclusion that contractionary monetary policy disproportionately reduces the supply of credit to riskier borrowers. For example, Jiménez et al. (2014) use changes in the overnight interest rate with a positive median, meaning that at least half of the observations have a positive change. Ioannidou, Ongena, and Peydró (2014) and Dell'Ariccia, Laeven, and Suarez (2017) use the federal funds rate in levels, which does not allow to distinguish sign-dependent effects. Paligorova and Santos (2017) use monetary policy surprises and changes in the federal funds rate, both with a positive median.

to riskier firms with potential rollover needs in response to monetary tightening because they are less constrained by the need to improve their capital ratios and can focus on mitigating potential losses from defaults on pre-existing loans.

Granular loan data from the euro area credit register over the recent monetary cycle allow me to overcome two key empirical challenges in identifying the effect of default risk on credit supply to firms with rollover needs. First, the dataset allows me to exploit variation in banks' internal probability of default (PD) estimates for the same firm within a given quarter. This variation, which stems from proprietary risk models unknown to the firm and its other lenders, improves upon publicly available credit ratings, which do not vary across banks. By focusing on firms that borrow from multiple banks, I can control for both observed and unobserved firm-level heterogeneity, including the possibility that riskier firms respond differently to monetary policy. That is, I exploit variation in banks' perceptions of firms' risk rather than their actual risk. By exploiting within firm-time variation, I cleanly identify a credit supply effect (Khwaja and Mian, 2008). The identification assumption is that the differential adjustment of credit demand to monetary policy changes of a firm that pays off identical shares of its loan volume at multiple banks in the same quarter does not depend on differences in banks' PD estimates.

Second, the data allow me to capture the timing of firms' rollover needs explicitly. The dataset has three key features. (i) It is available at the loan level, providing detailed information on the maturity structure of each firm's debt—a granularity not available in most credit registries, which are aggregated at the firm-bank-time level. (ii) The data track loans over time, a crucial feature because loan maturities can be adjusted after issuance. This panel dimension distinguishes the credit register from sources such as Dealscan and permits an accurate measurement of rollover needs by calculating the share of loan volume reaching maturity in any given quarter. (iii) In addition to loan volume, the dataset includes information on interest rates. This allows me to analyze loan volumes and prices, which further helps to disentangle credit demand from supply. In my analysis, I focus on firms that are predominantly financed by fixed-rate loans. These make up the majority of all euro area firms in my data (67.5 %). I exclude firms financed by floating rate loans because their interest rates are mechanically adjusted to monetary policy changes, which makes the effect of rollover much smaller and could contaminate my results.

Since monetary policy responds endogenously to economic conditions, I exploit its exogenous variation by using monetary policy surprises identified from high-frequency data around policy announcements. Jarociński and Karadi (2020) refine these surprises by removing information effects, thereby isolating the component attributable solely to monetary policy shocks. My results remain robust when using raw monetary policy surprises or changes in the ECB's policy rate as alternative measures. I combine the data with confidential supervisory data on banks' capitalization. This allows me to analyze heterogeneity across banks. In particular, I examine differences in the distance of banks to the capital requirements that would trigger restrictions on distributable amounts.

I find that when a risky firm pays off maturing debt following monetary tightening, its loan amount decreases less than that of a safer firm, while its interest rate rises less, i.e., it can roll over more debt on more favorable terms. The fact that loan amounts and interest rates move in opposite directions is consistent with a supply-side effect. Consider a firm that pays off all its loans with a bank in a given quarter: a one standard deviation monetary policy shock reduces the growth rate of total loans by 2.0 percentage points less for a firm at the 90th percentile of the PD distribution compared to one at the 10th percentile, and its interest rate increases by 3.3 basis points less. The estimates are about 1.5 times larger when considering contractionary shocks only. To examine heterogeneity across banks, I split the sample into banks relatively close to their capital requirements and banks further away. I find that the effect is about two to three times larger for banks with more capital headroom. In addition, I document a nonlinear effect of default risk - consistent with Faria-e-Castro, Paul, and Sánchez (2024) - that is most pronounced for firms in the top decile of the PD distribution. When I extend the sample to include single-bank borrowers and firm-time fixed effects with industry-location-size-time fixed effects (Degryse, De Jonghe, Jakovljević, Mulier, and Schepens, 2019), I obtain similar results in this larger sample.

The magnitude of the effect is economically significant. To illustrate the importance of evergreening in the context of monetary tightening, I conduct two exercises. First, I show that about 40% of the volume of new loan contracts is used to roll over maturing loans, indicating that evergreening incentives influence a substantial share of new lending activity. Second, I conduct a partial equilibrium back-of-the-envelope calculation. This exercise compares a scenario in which banks do not adjust credit supply to monetary policy depending on borrower default risk with one in which they do. The findings indicate that banks' evergreening behavior documented in this paper reduces the financing shortfall induced by monetary tightening by up to 30% for a firm at the 90th percentile of the PD distribution.

I rule out alternative explanations for my findings. In particular, I demonstrate that banks adjust their credit supply in response to individual borrower risk rather than to differences in aggregate portfolio credit risk. Moreover, I show that the results are not driven by differences in maturities or known biases in PD estimates (Firestone and Rezende, 2016; Berg and Koziol, 2017; Plosser and Santos, 2018; Behn, Haselmann, and Vig, 2022).

My findings have important policy implications. I show that the transmission of monetary policy through bank lending is muted in the presence of maturing debt for riskier firms. In the short run, the evergreening behavior of lenders may mitigate financial stability concerns by reducing the likelihood of defaults for firms facing heightened interest expenses due to monetary tightening. In the longer run, however, my results suggest that the share of riskier lending tends to increase after such tightening, and the credit risk associated with these risky loans may not be accurately reflected in pricing. This complements the findings of Grimm, Jordà, Schularick, and Taylor (2023), who show how expansionary monetary policy can increase financial fragility. The fact that the effect is stronger for banks further away from capital requirements mitigates the concerns to some extent. It also implies that solid bank capitalization helps risky firms weather difficult periods such as sharp monetary tightening.

Related literature. This paper contributes to four strands of the literature. First, I add to the literature on evergreening by showing how lenders' evergreening behavior intensifies under monetary tightening. Faria-e-Castro, Paul, and Sánchez (2024) document that lenders have incentives to offer favorable terms to firms close to default. They present model-based evidence indicating that evergreening negatively impacts overall productivity in the economy. My results suggest that the extent of these adverse effects is contingent on the stance of monetary policy. Combining their findings with mine implies that when monetary policy tightens, productivity declines more due to elevated incentives for evergreening. In addition, the literature on zombie lending and monetary policy could be seen as related to my findings. However, Faria-e-Castro, Paul, and Sánchez (2024) clearly distinguish evergreening from zombie lending, both theoretically and empirically. They document that many zombie firms have a very low PD. Zombie firms rather are characterized by low productivity, which may not necessarily translate into a high PD. While Acharya, Eisert, Eufinger, and Hirsch (2019) document how the ECB's Outright Monetary Transactions program supported lending to zombie firms, their focus is on zombie rather than firms with higher default risk and on a single unconventional monetary policy event. Similarly, Albuquerque and Mao (2023) find that higher sovereign bond yields induce more lending to zombie firms but do not distinguish between true new lending and rollover dynamics. My data allow me to address this gap, and I show how lenders relatively increase credit supply to riskier firms during monetary tightening when existing debt matures.

Second, I contribute to the literature on how the interaction between borrower risk and monetary policy affects lender behavior. Several papers document that loose monetary policy encourages banks to lend more aggressively to riskier borrowers in the context of *new* lending business (Ioannidou, Ongena, and Peydró, 2014; Jiménez et al., 2014; Dell'Ariccia, Laeven, and Suarez, 2017; Paligorova and Santos, 2017).[3] In contrast, I demonstrate that when firms have outstanding debt with banks, banks' lending behavior under monetary tightening differs from the traditional risk-taking channel. Faced with potential losses on existing exposures, banks moderate their credit cuts for riskier firms and extend more favorable loan terms in response to

3. For example, Jiménez et al. (2014) explicitly focus on cases where a new bank-firm relationship is established.

monetary tightening. Unlike the effects of the traditional risk-taking channel, which results in an external financing shortfall that limits a firm's ability to invest in new projects, tighter loan terms for firms with potential rollover needs create an internal cash shortfall. This reduction in equity and liquidity, in addition to higher interest expenses, could push ex-ante risky firms over the edge of default.[4] Because existing debt alters lenders' incentives in this way, the mechanism operates in the opposite direction than the traditional risk-taking channel: Riskier firms receive more credit following monetary tightening. This underscores the importance of considering the credit history of firms and lenders to understand how monetary policy transmission depends on borrower debt, as credit supply decisions do not occur in a vacuum. In contrast to Jiménez et al. (2014), my results are stronger for *high* capitalized banks with the necessary lending capacity to engage in evergreening.

Third, I contribute to the literature on how monetary policy affects firms when they roll over debt. While Deng and Fang (2022) and Jungherr, Meier, Reinelt, and Schott (2022) study the interaction of maturing debt and monetary policy, their papers are silent on heterogeneity along the risk dimension. This may be because they study corporate bond markets, where lending is much more dispersed than in bank lending, potentially reducing evergreening incentives. By actually measuring bank rollover and showing how lenders alleviate some of the burden of monetary tightening on riskier borrowers in the face of potential losses on existing debt, I document important heterogeneity. While papers have examined the effects of maturing debt during financial crises (Brunnermeier and Yogo, 2009; Acharya, Gale, and Yorulmazer, 2011; Almeida, Campello, Laranjeira, and Weisbenner, 2012), this paper goes beyond these analyses by showing how the impact of debt maturities and rollovers varies outside of crisis periods due to monetary policy.

Fourth, I contribute to the literature on the role of relationship lending in monetary policy transmission. Hachem (2011) and Berger, Bouwman, Norden, Roman, Udell, et al. (2024) show that the existence of a relationship affects the transmission of monetary policy from lenders to borrowers. My results complement these findings by showing that, conditional on the existence of a lender-borrower relationship, the associated credit risk significantly influences the response of credit supply to monetary policy.

The remainder of the paper proceeds as follows. In section 1.2, I develop the main argument. Section 1.3 describes the data. In section 1.4, I describe how I identify a credit supply effect to firms depending on their default risk and present the empirical results. In section 1.5, I present two exercises to quantify the importance of the mechanism I document. In section 1.6, I discuss how alternative explanations do not drive my results. The last section concludes.

4. The focus on the *internal* financing shortfall is similar to the effect pointed out by Ippolito, Ozdagli, and Perez-Orive (2018) for the floating-rate channel of monetary policy.

1.2 Hypothesis Development: Firms' Need for Rollover, Lenders' Evergreening Incentives, and Monetary Policy

In this section, I develop the hypotheses that guide my empirical analysis by combining insights on banks' responses to monetary policy with evergreening incentives.

Monetary tightening creates incentives for banks to reduce credit supply. As banks' financing costs tighten, they typically pass at least some of these costs on to borrowers. This mechanism is known as the bank lending channel and is well-documented in the literature. Firms with rollover needs should be particularly exposed to this channel, as they rely on new loan contracts to roll over maturing debt. This leads to my first hypothesis:

Hypothesis 1. *A tighter monetary policy leads to a reduction in bank credit supply for firms with rollover needs.*

A reduction in credit supply creates an internal financing gap for firms that pay off maturing debt. When firms pay off maturing loans, three scenarios are possible: (i) the firm rolls over the loans and has to fund higher interest expenses from liquidating assets; (ii) the firm pays off the loans out of some liquid assets, reducing equity and raising the likelihood of bankruptcy in the future; or (iii) the firm goes bankrupt directly if doing so is more advantageous than accepting the new loan terms.[5] Faria-e-Castro, Paul, and Sánchez (2024) demonstrate that loan terms, i.e., credit supply, are critical to the bankruptcy decision of riskier firms. Tighter loan terms make it more likely that the firm decides against (i) and either goes bankrupt directly, as in (iii), or has a higher future bankruptcy likelihood, as in (ii). When loan terms deteriorate, firms paying off maturing debt face higher costs without receiving any additional revenues. This mismatch creates an internal financing gap that increases the firm's financial vulnerability.

Firms with a higher ex-ante probability of default are particularly vulnerable to financing gaps. Firms that are already closer to default might be pushed over the edge by the additional financing gap. In contrast, firms with low default risk are better equipped to handle financing shortfalls, as they have greater flexibility to adjust their financing strategies without jeopardizing their financial health.

Since banks are willing to provide more favorable loan terms to avoid realizing losses from firms defaulting on existing debt, they should reduce credit supply less to risky firms with rollover needs in response to monetary tightening. Banks have incentives to adjust loan terms to firms close to default to avoid realizing costly losses from the default of these firms, i.e., to evergreen (Faria-e-Castro, Paul, and Sánchez, 2024). As a uniform reduction of credit supply in response to monetary tightening

5. Faria-e-Castro, Paul, and Sánchez (2024) do not consider the second scenario in their evergreening model.

would be more severe for firms already closer to default, evergreening incentives lead banks to adjust their credit supply reductions during monetary tightening based on the likelihood of triggering a firm's default. However, this behavior relies on the presence of existing debt with the firm, as banks aim to minimize losses from existing exposures. This mechanism does not apply to new lending businesses, as studied, for example, in Jiménez et al. (2014). Banks tend to shield ex-ante riskier firms from the full reduction in credit supply triggered by monetary tightening, provided they hold outstanding debt with these firms. This selective adjustment helps banks avoid realizing losses associated with defaults on existing loans. I will test this hypothesis empirically:

Hypothesis 2. *Banks reduce credit supply to firms with rollover needs less in response to tighter monetary policy if the firms are closer to default.*

The effect should be particularly pronounced in the case of monetary tightening rather than monetary easing. Reducing credit supply disproportionately increases the likelihood of bankruptcy for already vulnerable firms, which banks have an incentive to avoid if they hold existing debt with these firms. By selectively limiting the reduction in credit supply during monetary tightening, banks mitigate the risk of realizing costly losses from defaults. In contrast, monetary easing works in the opposite direction, as an increase in credit supply reduces the default probability of vulnerable firms. However, banks have little incentive to interfere with this process by increasing credit supply less to these firms. While looser monetary policy typically improves loan terms for all firms, banks have no strong reason to expand credit supply to the most vulnerable firms disproportionately. This asymmetry leads to my third hypothesis:

Hypothesis 3. *The effect is stronger for monetary tightening than for monetary easing.*

Evergreening incentives differ across banks. Capital headroom - defined as the amount of core equity tier 1 (CET1) capital above regulatory requirements - should play a critical role in shaping banks' incentives towards firms' rollover needs.[6] I measure capital headroom relative to risk-weighted assets (RWA) because risk-weighted capital ratios are the focus of most bank regulation today.[7]. Once banks deplete their capital headroom, they reach the minimum distributional amount (MDA) trigger; at this point, supervisors typically intervene - for example, by imposing limits on dividend payments or bonuses. To avoid breaching the MDA trigger, banks generally maintain capital targets above the regulatory minimum (Couaillier, 2021). Gropp,

6. Capital requirements consist of minimum requirements and buffer requirements. Couaillier (2021) and Couaillier, Lo Duca, Reghezza, and Rodriguez D'Acri (2025) document that banks react similarly when approaching these thresholds.

7. An exception is the leverage ratio, which is based on total assets and became binding in the euro area in 2021.

Mosk, Ongena, and Wix (2018) demonstrate that banks that want to increase capital reduce lending to increase the capital ratio for a given amount of CET1. In particular, they cut exposures with higher risk weights (Couaillier, 2021; Couaillier et al., 2025), which has a more pronounced effect on the capital ratio. The ratio is defined as CET1 ratio $= \frac{\text{CET1}}{\text{RWA}}$.

When a bank faces a firm with rollover needs, there are three possible outcomes:

1. The bank and the firm agree on a rollover, leaving both CET1 and RWAs unchanged, i.e., ΔCET1 ratio $= 0$.
2. The bank and the firm do not agree on a rollover.
   a. With a certain probability, the firm then defaults. In this scenario, the bank incurs a loss on the loan - reducing its CET1 capital - while RWA decline as the maturing loan is removed. Under reasonable assumptions, this leads to a reduction in the CET1 ratio, i.e. ΔCET1 ratio < 0 (see Appendix section 1.B).
   b. With the complementary probability, the firm avoids default. In this case, only RWAs are reduced, i.e., ΔCET1 ratio > 0.

Banks with very little or even negative capital headroom (which are not present in my sample since all banks have some capital headroom) would want to avoid any reduction in CET1 at all costs and would therefore prefer rollover agreements (i.e., outcome (1)) regardless of the terms. In contrast, banks that have some capital headroom but are still relatively close to regulatory capital requirements face a trade-off between avoiding the loss by issuing a generous rollover loan (1) and the possibility of increasing their capital ratio by successfully enforcing repayment and getting rid of a high-risk weight loan (2b). Finally, banks with relatively more capital headroom do not face this trade-off; their primary concern is solely to avoid losses from default, as they would not benefit from increasing their CET1 ratio. Therefore, in particular, banks with more ex-ante capital headroom should cut lending to riskier firms with rollover needs less in response to monetary tightening. This leads to my fourth hypothesis:

Hypothesis 4. *The effect is stronger for banks with more ex-ante capital headroom.*

I focus on firms predominantly financed with fixed-rate loans, as floating-rate loans adjust mechanically to monetary policy. In the euro area, the vast majority of firms - about 67.5 % - rely primarily on fixed-rate loans. I focus on these firms in my analysis for two reasons. First, monetary policy changes mechanically affect the interest rate of floating rate debt through their impact on the reference rate underlying the floating rate. This channel could contaminate the effects of monetary policy on debt that I want to study. Second, floating-rate firms anticipate frequent interest rate changes and presumably manage their debt expenditures accordingly. Hence, evergreening motives might be less pronounced vis-à-vis these firms. In my sample, I only include firms with at least half of their debt volume in fixed-rate loans throughout the sample period. The choice between fixed and floating rate loans is to a significant extent determined by the country of the firm, as for example shown by (Core, De Marco, Eisert, and Schepens, 2024), and is therefore pretty stable over time.[8]

1.3 Data

In this section, I describe my data source, the euro area credit register, combined with supervisory information on bank capitalization. The granularity of the data allows me to identify the rollover needs of firms. I also describe how I measure the stance of monetary policy with high-frequency surprises and present summary statistics.

1.3.1 Data on Firms' Bank Loans

My primary data source is the harmonized euro area credit registry, which provides detailed information on corporate loans. In my main analysis, I use data from after the start of the COVID-19 pandemic to the most recent available, which is from Q1 2021 to Q4 2023.

AnaCredit (Analytical Credit Datasets), a comprehensive and confidential database maintained by the European Central Bank (ECB), was launched in September 2018. It harmonizes various national credit registers across euro-area countries. The database contains detailed loan-level information on all loans to legal entities with a reporting threshold of €25,000. The harmonization ensures consistency and comparability of credit data across different jurisdictions within the euro area.

I obtain loan-level micro data on loans issued by banks in all 19 euro area countries to euro area non-financial firms. Only very small banks are not included in my sample. While they report to the national central banks, their data is not forwarded to the European Central Bank for the euro area AnaCredit Europe. Throughout my analysis, I exclude firms that are in default. I describe the data cleaning and preparation process in more detail in Appendix section 1.C. AnaCredit covers a wide range of credit instruments, including overdrafts, trade receivables, financial leases, revolving credit, credit lines, and other loans of a non-revolving nature, such as term loans. In addition to harmonized bank and firm identifiers, the dataset provides granular details on loan-level attributes. In particular, I obtain information on the loan amount and interest rate, as well as whether the loan is fixed or floating and the maturity of the loan.

From the loan-level data, I construct a firm-bank-quarter panel by aggregating all loans that a given firm borrows from a given bank in a given quarter. I keep only

8. Core et al. (2024) report adjusted R^2 values in Table 2, but for a variance decomposition, the raw R^2 would be more appropriate, as their measure likely underestimates the variation explained by different fixed effects.

firms that have loans outstanding from at least one bank throughout the sample period. In the main sample, I use data from Q1 2021 to Q4 2023. Even though data collection for AnaCredit started in 2018, I start my analysis in 2021 to avoid my effects being contaminated by the start of the Covid-19 pandemic and government guarantee programs. The end of the sample is determined by the availability of monetary policy shocks, which I will discuss below. As described above, in the main analysis, I focus on firms primarily financed with fixed-rate loans.

1.3.2 Determining Firms' Rollover Needs

The granularity of the loan-level panel data allows me to measure when maturing loans are paid off, creating rollover needs for firms.

While many theoretical studies emphasize the importance of corporate debt rollover (e.g. Acharya, Gale, and Yorulmazer, 2011; He and Xiong, 2012), empirical research on the topic remains scarce. This may be due to the specific data requirements needed to study rollover in bank lending. Such requirements are often met by the granular data available on corporate bonds, allowing Deng and Fang (2022) and Jungherr et al. (2022) to study the effect of monetary policy on firms depending on their maturity structure. However, the corporate bond market differs substantially from bank lending, particularly concerning evergreening motives. In bond markets, lending is more dispersed.

The granularity of the data in the euro area credit registry, AnaCredit, allows me to study rollover needs by meeting key data requirements that are typically missing from most other loan datasets. First, the data must be at the individual loan level. Panel data at the firm-bank-time level aggregates loan volumes, making it impossible to identify new loans and repaid loans. Only with individual loan-level data can one accurately track loan origination, repayment, and maturity structure. Second, the data must have a panel structure. Transaction-level data, which record loans only at origination, are insufficient because they do not allow to determine the date of loan repayments. Although the original maturity is often available, loans may be renegotiated (Roberts, 2015) or repaid early (Mian and Santos, 2018). This means that assuming the debt would have to be rolled over at the originally agreed maturity date would introduce a significant bias. In addition to tracking individual loans over time, AnaCredit offers a critical advantage: unlike most credit registries, it also includes monthly interest rate information for each loan. While not essential for measuring rollover needs, this feature allows me to disentangle credit supply and demand effects in addition to studying within firm-time variation by analyzing changes in interest rates and loan amounts together.

In the empirical analysis, I use the share of loans that are paid off at maturity in total outstanding loan amount as a measure of rollover needs. Firms do not always plan to roll over maturing loans. Instead, they sometimes intend to repay them and reduce their debt. To address this concern, I compare credit supply for the same firm borrowing from different banks at the same time. Furthermore, for each firm, there exists some interest rate at which it is more profitable to roll over the loan rather than to repay it. Let $Loans_{i,b,t}$ be the total loan amount that firm i borrows from bank b in quarter t, and $Paid\ off\ loans\ at\ maturity_{i,b,t}$ be the loans borrowed by firm i from bank b that were reported in $t-1$ for the last time and, thus, paid off in quarter t. I restrict this to loans with maturity dates up to four quarters before or at the last day of quarter t to exclude early repayments.[9], then I define[10]

$$\text{Rollover need}_{i,b,t} := \frac{\text{Paid off loans at maturity}_{i,b,t}}{\text{Loans}_{i,b,t-1}} \tag{1.3.1}$$

In robustness checks, I study alternative ways to define rollover needs.

I use the rollover needs arising from maturing loans rather than the volume that is actually rolled over. The amount actually rolled over reflects an equilibrium outcome, which makes it inappropriate to analyze the effects of credit supply.

1.3.3 Data on Banks' Capitalization

I combine the loan data with supervisory data on banks for the lagged quarter. The data, which the banks' supervisors also use, is available quarterly for all banks directly or indirectly supervised by the Single Supervisory Mechanism. I always refer to the highest consolidation level within the euro area for banking groups. I use the capital headroom, which is available for most banks in my sample.

1.3.4 Measuring Monetary Policy

My main measure of monetary policy is the high-frequency monetary policy shock series from Jarociński and Karadi (2020), which extract the surprise component of the ECB's monetary policy decisions. I show that there is significant variation in this measure. I use the raw surprises for robustness. As an alternative measure, I use changes in the deposit facility rate. This alternative measure is correlated over time.

Since monetary policy reacts endogenously to economic developments that may affect credit supply, I rely on monetary policy shocks identified from high-frequency data to identify the pure effect of monetary policy. In particular, I use the shock series provided by Jarociński and Karadi (2020).[11] Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019) provide the changes in overnight indexed swaps (OIS) for different maturities around each ECB policy decision. Jarociński and Karadi (2020)

9. Additionally, I require the maturity date to be not before the last quarter.

10. I use paid off rather than maturing loans to allow for some flexibility in the exact timing of repayments. Loans that paid off a few quarters before maturity are most likely paid off because they are approaching maturity.

11. I downloaded the data from https://github.com/marekjarocinski.

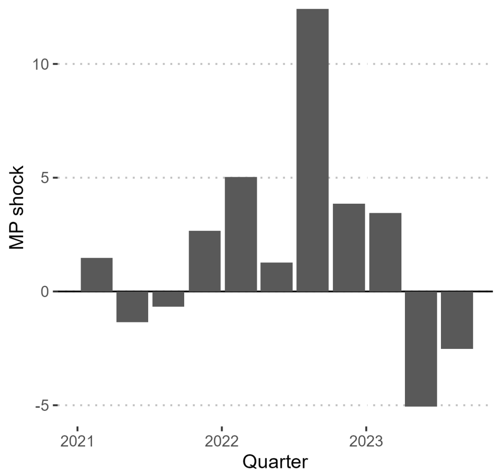


Figure 1.3.1. Monetary Policy Shocks Over Time

Notes: This figure plots the main measure of monetary policy over time. *MP shock*$_t$ is the sum of all high-frequency monetary policy surprises in a quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps.

compute the first principle component of different shock series for OIS with maturities of 1-, 3-, 6-months, and 1-year. Afterward, they estimate a vector autoregressive model with sign restrictions to purge the first principle component from changes due to information surprises in the ECB decision. The remaining series measures unexpected changes in the monetary policy stance. I sum all shocks in a quarter as my main monetary policy measure, $MP\ shock_t$, in basis points. The measure is available through Q3 2023. I use the lagged value of the shock series to rule out reverse causality. This allows me to end my sample in Q4 2023. There is considerable variation in this measure. Figure 1.3.1 shows the shock time series for each quarter in the sample. The measure varies in both magnitude and sign, even during the period when monetary policy was at the zero lower bound. I will later also use a measure of only contractionary monetary policy shocks, which I calculate as

$$\text{MP shock}_t^{\geq 0} = \begin{cases} \text{MP shock}_t & \text{if MP shock}_t \geq 0 \\ 0 & \text{else} \end{cases} \tag{1.3.2}$$

I also show robustness for using the raw monetary policy surprise without any information purging.

I use the quarter-on-quarter change in the ECB's deposit facility rate (DFR) as an alternative measure of monetary policy, ΔDFR_t, in basis points. Of the three ECB policy rates, I use the DFR to measure the monetary policy stance because it was the most important of the three ECB policy rates over the sample horizon due to the high level of banks' excess reserves with central banks (Banco de España, 2023;

Table 1.3.1. Summary Statistics: Firm-Bank-Quarter Panel

	Mean	SD	P25	P50	P75	N
Rollover need	0.02	0.11	0.00	0.00	0.00	4,027,180
MP shock	1.6	4.3	-1.4	1.5	3.9	4,027,180
MP shock$^{\geq 0}$	2.6	3.4	0.0	1.5	3.9	4,027,180
PD estimate	3.1	7.4	0.4	1.0	2.4	4,027,180
Capital headroom	5.7	2.4	4.0	6.2	6.9	3,919,710
Loans	905.1	8,169.7	58.5	148.2	389.9	4,027,180
Δ log Loans	-2.7	44.5	-8.8	-4.7	-1.1	4,027,180
Interest rate	193.2	164.5	89.8	144.9	242.3	3,937,738
Δ Interest rate	4.7	44.0	-0.0	0.0	0.3	3,919,003
Number of bank relations	2.6	1.2	2.0	2.0	3.0	4,027,180
$\frac{\text{Loans}_{i,b,t}}{\text{Loans}_{i,t}}$	0.4	0.3	0.2	0.4	0.6	4,027,180
Rollover need$_{i,t}$	0.02	0.08	0.00	0.00	0.00	4,027,180

Notes: This table reports summary statistics for firm *i* borrowing from bank *b* in quarter *t*. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *MP shock*$^{\geq 0}_{t-1}$ is defined in equation (1.3.2). *PD estimate* is the bank's individual lagged estimate of the firm's risk to default within the next year, measured in percentages. *Capital headroom*$_{b,t-q}$ is the bank's lagged capital above combined regulatory requirements, measured in percentage points. Loans are measured in thousands. The quarter-on-quarter log difference in the loan amount is multiplied by 100. Interest rates are measured in bps. *Rollover need*$_{i,t}$ is defined in equation (1.4.3).

Välimäki, 2023). Such a direct measure of monetary policy has three advantages. First, it allows for a more straightforward interpretation of the magnitudes. Second, it is also available after Q3 2023, which allows me to capture the current monetary cycle. Since the DFR remained unchanged until July 2022, I build a different firm-bank-quarter panel for this analysis. It starts later, namely in Q2 2022, and ends with the latest available data in Q3 2024. Third, recent studies have questioned the relevance of high-frequency monetary policy surprises as an instrument for monetary policy changes in the U.S. (Bauer and Swanson, 2023a; Bauer and Swanson, 2023b). If this is also a concern for the euro area, observed policy rates can serve as an informative cross-check.

This alternative measure is autocorrelated. Appendix Figure 1.A.1 shows the time series, which already suggests some autocorrelation. In fact, the autocorrelation of the change in the DFR is 0.64 with a p-value of 5%. I will control for this in the regressions by including an additional lag.[12]

12. In contrast, the autocorrelation of the shock series is 0.22 with a p-value of 51%. This clearly insignificant and relatively low correlation coefficient alleviates concerns about autocorrelation in this series. Moreover, by construction, monetary policy surprises should not be autocorrelated.

Table 1.3.2. Variation in the share of how much paid off loans are rolled over

Dependent variable:			Share rolled over$_{i,b,t}$			
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.38***					
	(0.01)					
Bank-Time FE	No	Yes	No	No	No	Yes
Firm-Bank FE	No	No	Yes	No	Yes	Yes
Firm-Time FE	No	No	No	Yes	Yes	Yes
R^2		0.18	0.53	0.58	0.85	0.86
Observations	398,871	398,638	314,413	108,397	93,267	93,216

Notes: This table shows various fixed effects regressions to explain the share of paid-off loans that are rolled over between firm i and bank b in quarter t. The sample includes those observations from a firm-bank-quarter panel from Q1 2021 to Q4 2024 for all multibank euro-area firms with some bank debt in all quarters and predominantly financed with fixed-rate loans for which the firm paid off some maturing loans in the current quarter. The dependent variable is defined as *Share rolled over*$_{i,b,t} = \frac{\text{Rolled over}_{i,b,t}}{\text{Paid off loans at maturity}_{i,b,t-1}}$. *Rolled over*$_{i,b,t}$ is defined as min(new loans$_{i,b,t}$, paid off loans at maturity$_{i,b,t}$). *Paid off loans at maturity*$_{i,b,t}$ denote the volume of all loans borrowed by firm i from bank b that were reported in $t-1$ for the last time and, thus, paid off in quarter t. Standard errors in column (1) are clustered at the firm-bank and quarter level.

1.3.5 Descriptive statistics

In this section, I present summary statistics for the firm-bank-quarter panel. Most firms have few bank relationships, which makes evergreening incentives likely.

The sample consists of about 4 million observations. The sample contains 210,582 unique firms and 302 unique banks over the 12 quarters. The mean (median) number of firm relationships per bank is 1271 (245).

Table 1.3.1 provides summary statistics for the firm-bank-quarter panel of multibank firms predominantly financed with fixed-rate loans over the period from Q1 2021 to Q4 2023. On average, firms have a *rollover need*$_{i,b,t}$ of 2% of their total loans. The mean of *MP shock*$_{t-1}$ is positive with 1.6bps and has a standard deviation of 4.3. In the empirical analysis, I will use *PD estimate*$_{i,b,t-1}$ as a measure of default risk. The mean of this measure is 3.1% with a substantial standard deviation of 7.4. I discuss this measure in more detail below. Reflecting the predominantly monetary tightening period captured in the sample, *Loans*$_{i,b,t}$ declined on average while *Interest rate*$_{i,b,t}$ increased.

Bank lending in the euro area is characterized by a limited number of bank relationships for most firms, as shown in the last two rows of Table 1.3.1 and consistent with the findings of Kosekova, Maddaloni, Papoutsi, and Schivardi (2023). For the median firm-bank relationship in the sample, the firm borrows from only two banks, and a single bank provides about 40% of the firm's total loans. This lending structure is consistent with evergreening incentives.

There is significant unexplained variation in the fraction of maturing loan volume that is actually rolled over. This suggests that banks do not follow a uniform policy but instead adjust credit supply both across firms and over time. Table 1.3.2 quantifies how much of the variation in the share of paid-off loans that are actually rolled over is explained by various fixed effects. In this table, I use the share of paid-off loan volume that is actually rolled over rather than the rollover need, which is the focus of the rest of the paper, to demonstrate some variation in the outcome of equilibrium effects. The analysis is restricted to observations where firms have paid off some loans. Column (1) shows that, on average, firms roll over about 38% of the amount they pay off at maturity. Column (2) indicates that there is substantial variation across firms paying off loans with the same bank in the same quarter, as bank-quarter fixed effects explain only 18% of the variation. This suggests that a considerable share of the variation stems from differences in rollover demand across firms or from banks tailoring their credit supply to firms with rollover needs (or both). Column (3) highlights significant variation over time within firm-bank pairs, as firm-bank fixed effects account for only about half of the observed differences. Similarly, column (4) shows that even when looking at the same firm in the same quarter, there is substantial variation in rollover rates across different banks for firms that pay off loans to multiple banks. Combining firm-bank and firm-quarter fixed effects, column (5) provides evidence for time-varying credit supply, as 15% of the variation remains unexplained after controlling for firm demand and time-invariant firm-bank heterogeneity. Finally, column (6) includes all fixed effects and still leaves 14% of the variation unexplained, pointing to both changes in credit supply over time and differences in how banks adjust lending across borrowers. These findings reinforce the idea that banks adjust credit supply to firms that could roll over loans differently over time and to different borrowers. This motivates my empirical analysis.

1.4 Identification Strategy and Empirical Results

In this section, I present the key empirical findings of the paper. First, I describe how I identify a credit supply effect by exploiting variation in PD estimates across banks. Then, I document the mitigating effect of higher default on banks' credit supply responses to monetary policy for firms with rollover needs, i.e. the evergreening behavior induced by monetary tightening. Notably, this effect is more pronounced for contractionary monetary policy and also holds when including single bank firms in the sample. Additionally, the results are similar when using the DFR as an alternative to monetary policy shocks. Banks with larger distances to capital requirements engage more in this lending practice. I conclude this section by providing some evidence on the non-linear impact of default risk.

1.4.1 Estimating the Effect of Monetary Policy on the Credit Supply to Firms With Rollover Needs

I start the empirical analysis by examining how monetary policy affects credit supply to firms with rollover needs. To test *Hypothesis 1* that a tighter monetary policy leads to a reduction in bank credit supply for firms with rollover needs, I estimate the following regression:

$$\begin{aligned} \Delta y_{i,b,t} = &\alpha_1 \text{Rollover need}_{i,b,t} \\ &+\alpha_2 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \\ &+\eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t} \end{aligned} \tag{1.4.1}$$

for firm i borrowing from bank b in quarter t. The dependent variable $\Delta y_{i,b,t}$ represents either the quarter-on-quarter change in the log total loan volume

$$\Delta \log(\text{Loans})_{i,b,t} = \log(\text{Loans})_{i,b,t} - \log(\text{Loans})_{i,b,t-1},$$

or the quarter-on-quarter change in the volume-weighted average interest rate,

$$\Delta \text{ Interest rate}_{i,b,t} = \text{Interest rate}_{i,b,t} - \text{Interest rate}_{i,b,t-1}.$$

α_1 estimates the average effect of paying off maturing loans in the absence of monetary policy shocks. I expect $\alpha_1 < 0$ when the dependent variable is the loan amount, as firms do not roll over all paid-off loans. I am agnostic about the effect on interest rates. α_2 estimates the average effect of monetary policy on credit supply to firms with rollover needs, thereby providing an empirical test for *Hypothesis 1*. According to the bank lending channel (Kashyap and Stein, 2000), tighter monetary policy reduces the supply of bank credit. As firms with rollover needs are affected by credit supply, I expect $\alpha_2 < 0$ when the dependent variable is the loan amount and $\alpha_2 > 0$ for the interest rate. I discuss the role of the various fixed effects below.

1.4.2 Identifying a Credit Supply Effect to Riskier Firms

In this section, I describe my identification strategy. I leverage banks' individual default risk estimates to measure their private perception of a firm's credit risk. For the same firm, estimates vary across banks and within the same bank over time. To identify a credit supply effect, I control for credit demand by using fixed effects and by studying the joint dynamics of loan amounts and interest rates.

To identify a credit supply effect that varies with default risk and test *Hypothesis 2*, I exploit variation in PD estimates for the same borrower at the same time across banks. Using a single PD estimate per firm-time would risk confounding supply effects with differential responses of firms to monetary policy based on their default risk.[13] In AnaCredit, banks are required to report their estimates of the probability that the borrower will default within the next year (European Central Bank, 2019, pp 253). These PD estimates are derived from banks' internal credit risk models, which must be validated by supervisors and are used to calculate risk weights under Basel capital regulations. Because these estimates are model-based and specific to each bank, they can vary across banks for the same borrower. I denote the estimate of the default risk of firm i by bank b in quarter t as $\textit{PD estimate}_{i,b,t}$. For these PD estimates to identify a differential credit supply effect for riskier firms, two conditions are essential: (i) there must be substantial variation across banks, and (ii) this variation does not have to be systematic. I discuss both requirements next.[14] [15]

PD estimates vary across banks and within the same bank over time for the same borrower. Table 1.4.1 shows results of various approaches to explain the PD estimate reported by bank b for firm i in quarter t. Column (1) shows that within the three years of my sample, there is considerable variation in the PD estimate for the same firm by the same bank, as firm-bank fixed effects explain only 42% of the PD variation. Column (2) demonstrates that while PD estimates are highly autocorrelated, they can still change significantly even from one quarter to the next, with the lagged PD estimate explaining only 47% of the variation. Column (3) reveals substantial variation in the PD estimates across banks for the same firm at the same time, with firm-quarter fixed effects explaining only 55% of the variation. Even when combining firm-bank and firm-quarter fixed effects in column (4), around 28% in the variation remains unexplained. PDs naturally also vary within a bank, as demonstrated in column (5). Even combining all fixed effects in column (6) leaves 22% of the variation unexplained.

The literature has highlighted substantial variation in banks' PD estimates and documented that cross-bank differences are mainly idiosyncratic, presumably driven by differences in models. For example, Berg and Koziol (2017) document large vari-

13. For example, Ottonello and Winberry (2020) show that firms are more responsive to monetary policy when they have low default risk, i.e., low debt burden and high distance to default.

14. In addition, I require first that firms do not know the PD estimates assigned to them by different banks. This assumption is plausible, as internal credit risk models are an integral part of a bank's business model and are not disclosed externally. Second, I require banks to incorporate these PD estimates into their lending decisions, even though the estimates are calculated primarily for regulatory purposes. For example, the Capital Requirements Regulation assumes that banks use the same estimates for calculating risk weights and for internal purposes per default, as they have to document if they do otherwise (European Parliament and Council of the European Union, 2013, Article 179). In any case, this concern would work against me.

15. Importantly, the quality of the PD estimates does not affect my analysis. I only require that the reported PD estimates are somehow predictive of a bank's internal risk management practices for that borrower. This mitigates concerns, for example, that the AnaCredit regulation requires banks to report regulatory PDs and, therefore, intended to be "through-the-cycle" PDs (TTC), which are supposed to be estimated independent of the current business cycle. In contrast, banks' internal risk management and loan pricing may depend on "point-in-time" PDs (PIT), which are also used for accounting purposes. However, as long as TTC and PIT PDs are correlated, they serve my purpose of providing information on the different relative risk perceptions of different banks for the same borrower at the same time.

Table 1.4.1. Variation in PD Estimates

Dependent variable:	PD estimate$_{i,b,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant		0.96^{***}				
		(0.25)				
PD estimate$_{i,b,t-1}$		0.61^{***}				
		(0.14)				
Firm-Bank FE	Yes	No	No	Yes	No	Yes
Firm-Time FE	No	No	Yes	Yes	No	Yes
Bank-Time FE	No	No	No	No	Yes	Yes
R^2	0.42	0.47	0.55	0.72	0.18	0.78
Observations	3,987,884	4,005,824	3,979,406	3,973,031	4,005,704	3,972,917

Notes: This table shows how different fixed effect explain variation in PD estimates by bank b for firm i in quarter t. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. Standard errors in column (2) are clustered at the firm-bank and quarter level.

ability in PD estimates for the same borrower across banks. Similarly, Behn, Haselmann, and Vig (2022) provide evidence suggesting a relationship between bank capitalization and PD estimates. To address this concern, I conduct robustness checks by studying within bank-time variation in PD estimates. I discuss other potential identification challenges in section 1.6.

While not all banks are required to report PD estimates, they are available for the majority of observations. Mainly those banks that use internal models to calculate risk weights for regulatory purposes report PD estimates. A PD estimate is available for 78% of firm-bank-quarter observations.

To test *Hypothesis 2* that banks reduce credit supply to firms with rollover needs less in response to tighter monetary policy if they estimate them to have higher default risk, I extend equation (1.4.1) and interact the *PD estimate*$_{i,b,t-1}$ with the *Rollover need*$_{i,b,t}$ and the monetary policy measure *MP shock*$_{t-t}$. I estimate the following regression equation for the sample of firms that borrow from multiple banks:

$$
\begin{aligned}
\Delta y_{i,b,t} =\; & \beta_1 \text{Rollover need}_{i,b,t} \\
& + \beta_2 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \\
& + \beta_3 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_1 \text{PD estimate}_{i,b,t-1} \\
& + \gamma_2 \text{Rollover need}_{i,b,t} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_3 \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t}
\end{aligned}
\tag{1.4.2}
$$

for firm i borrowing from bank b in quarter t. Again, the dependent variable $\Delta y_{i,b,t}$ is either the quarter-on-quarter change in the log total loan volume or the quarter-on-quarter change in the volume-weighted average interest rate. I use the lagged value of *PD estimate*$_{i,b,t}$ to ensure that the rollover decision does not affect it. Banks are required to report "through-the-cycle" PDs, which should be based on long-run averages and, therefore, not affected by a one-quarter monetary policy shock. Nevertheless, I cannot rule out the possibility that *MP shock*$_{t-1}$ affects *PD estimate*$_{i,b,t-1}$. Therefore, I present robustness using *PD estimate*$_{i,b,t-2}$ below.

The main coefficient of interest is β_3, which captures the differential impact of monetary policy on firms with rollover needs that banks perceive as riskier, as evident from their higher *PD estimate*$_{i,b,t-1}$. A positive coefficient on loan volume and a negative coefficient on interest rates would indicate an increase in credit supply to such firms in response to monetary policy tightening, which would confirm *Hypothesis 2*. This would provide evidence for monetary tightening inducing evergreening behavior. This result would also suggest that the traditional risk-taking channel does not extend to cases where existing debt is a factor. In contrast, a negative coefficient on loan volume and a positive coefficient on interest rates would imply that the mechanism of the risk-taking channel carries over, with banks reducing credit supply more to riskier firms following monetary tightening, regardless of existing debt.

Importantly, firm-quarter fixed effects $\zeta_{i,t}$ absorb time-varying unobserved heterogeneity at the firm level, including credit demand (Khwaja and Mian, 2008). Identification of a differential credit supply effect depending on the PD, therefore, rests on the assumption that a firm paying off identical shares of maturing loans with multiple banks in the same quarter does not adjust its credit demand in response to monetary policy based on banks' PD estimates. The fact that PD estimates are generally not known to borrowers supports this assumption. Studying loan amounts and interest rates allows me to distinguish supply from demand shifts. If demand effects drove results, β_3 should have the same sign for both dependent variables. In contrast, if supply effects drive results, they should have opposite signs.

To also include firms with a single bank relationship, which account for a relevant share of euro area economies (Kosekova et al., 2023),I further estimate equation (1.4.2) replacing firm-time fixed effects with industry-sector-location-time fixed effects (Degryse et al., 2019). Identification of a differential credit supply effect depending on the PD then rests on the stronger assumption that firms in the same industry and region of the same size that are paying off identical shares of maturing loans with multiple banks in the same quarter do not adjust their credit demand in response to monetary policy based on banks' PD estimates.

Firm-bank fixed effects $\eta_{f,b}$ control for time-invariant variation across firm-bank pairs. Bank-quarter fixed effects $\xi_{b,q}$ take out time-varying observed and unobserved heterogeneity at the bank level. Since this would absorb all bank control variables, I do not include any. These fixed effects rules out, for example, that banks with riskier loans generally react differently to monetary policy. γ_1, γ_2 and γ_3 control for the effects of the remaining terms, i.e., the intercept term for the $PD\ estimate_{i,b,t-1}$, the interaction between $Rollover\ need_{i,b,t}$ and $PD\ estimate_{i,b,t-1}$, and the interaction between $MP\ shock_{t-1}$ and the $PD\ estimate_{i,b,t-1}$.

I note that even if there was no cross-bank variation in $PD\ estimate_{i,b,t-1}$, β_3 in equation (1.4.2) could still be estimated from the variation in $Rollover\ need_{i,b,t}$ if the credit supply response to monetary policy through rollover needs would depend on the average PD estimate of a firm at a given point in time. To address this issue, I estimate additional regressions in which I replace the bank-specific rollover need with the aggregate rollover need of a firm, defined as

$$\text{Rollover need}_{i,t} := \frac{\text{Paid off loans at maturity}_{i,t}}{\text{Loans}_{i,t-1}} \tag{1.4.3}$$

Even though I use the lagged PD estimate, if PDs reflected banks' future lending decisions, this would, if anything, bias my estimates downward. If banks already anticipate their evergreening behavior and therefore assign a lower PD to actually riskier firms, it would make it less likely that I find a significant estimate on β_3. In this sense, my empirical estimates would report lower bounds for the true effect.[16]

1.4.3 Empirical Results on the Impact of the Default Risk of Borrowers With Rollover Needs on Lenders' Credit Supply Responses to Monetary Policy

In this section, I present empirical estimates of the regression equations to test the hypotheses I derived above. In particular, I show that monetary policy affects the credit supply to firms with rollover needs but less so if they are closer to default. This risk-dependent impact is stronger for contractionary monetary policy.

Monetary tightening leads to a reduction in credit supply to firms with rollover needs. Columns (1) and (5) of Table 1.4.2 show empirical estimates of equation (1.4.1) without firm-time fixed effects. The negative and significant estimate for α_1 in column (1) confirms that a firm's total loan amount decreases when it pays off maturing loans that create rollover needs, mirroring that firms do not roll over all maturing loans. On average, a firm that repays all of its loans, i.e., has a $Rollover\ need_{i,b,t} = 1$, experiences a 46 pp. lower growth rate in $Loans_{i,b,t}$. This effect is amplified by monetary policy: The negative estimate for α_2 indicates that loan growth is lower after monetary policy shocks. A one standard deviation monetary policy shock (4.3bps) results in an additional 4.4 pp. reduction in loan growth. The results in column (5) confirm that credit supply responses to monetary policy drive this, as the positive estimate of α_2 shows that interest rates for firms with rollover needs simultaneously increase more after a monetary policy shock. This confirms *Hypotheses 1*. Specifically, a one standard deviation higher monetary policy shock leads to an

16. Faria-e-Castro, Paul, and Sánchez (2024, p. 7) argue in a similar way.

Table 1.4.2. Impact of Default Risk Estimate on Credit Supply Response to Monetary Policy

Dependent variable:	Δ log Loans$_{i,b,t}$				Δ Interest rate$_{i,b,t}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollover need$_{i,b,t}$	-46.16***	-46.06***	-46.36***	-46.32***	4.64	4.77	3.11	3.28
	(3.70)	(3.66)	(4.06)	(4.01)	(5.29)	(5.00)	(5.38)	(5.15)
Rollover need$_{i,b,t}$ × MP shock$_{t-1}$	-1.02**	-1.17***	-1.24***	-1.40***	4.38***	3.61***	4.79***	4.00***
	(0.34)	(0.33)	(0.38)	(0.37)	(0.79)	(0.64)	(0.90)	(0.72)
Rollover need$_{i,b,t}$ × MP shock$_{t-1}$ × PD estimate$_{i,b,t-1}$			0.08**	0.08***			-0.15**	-0.13***
			(0.03)	(0.03)			(0.05)	(0.04)
Firm-Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	No	Yes	No	Yes	No	Yes	No	Yes
Remaining interaction terms	No	No	Yes	Yes	No	No	Yes	Yes
Adjusted R^2	0.02	-0.09	0.02	-0.10	0.04	-0.04	0.04	-0.04
R^2	0.14	0.51	0.14	0.51	0.16	0.53	0.16	0.54
Observations	4,010,825	4,006,751	4,010,825	4,027,180	3,902,154	3,833,287	3,902,154	3,852,172

Notes: Columns (1), (2), (5), and (6) show empirical estimates of equation (1.4.1) while the remaining columns show empirical estimates of equation (1.4.2). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first four columns is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in the remaining columns is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. In columns (3), (4), (7), and (8), the remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

18.8 bps higher interest rate change. Columns (2) and (6) show that the estimates are robust to including firm-time fixed effects. This finding underscores the intuitive notion that firms with rollover needs are more exposed to monetary policy through banks' credit supply. Monetary policy induces an internal financing gap for those firms that are paying off maturing debt. Consequently, this can create incentives for banks to engage in evergreening, i.e., provide relatively better loan terms to firms closer to default, for which the reduction in credit supply is more dangerous. This is what I test next.

Banks reduce credit supply less to risky firms with rollover needs in response to monetary tightening. Columns (3) and (7) show empirical estimates of equation (1.4.2) without firm-time fixed effects. In addition to the previous results, I fully interact the right-hand side variables with *PD estimate*$_{i,b,t-1}$. The coefficient of interest is now β_3 on the triple interaction, which is statistically significant in both columns. The estimate is robust to including firm-time fixed effects in columns (4) and (8). Column (4) shows that the loan amount of firms with rollover needs for which banks estimate a higher PD decreases less in response to monetary policy. A one interquartile range, i.e., 2pp, higher PD attenuates the effect of the monetary policy shock on the growth rate of total loans by 11%. Appendix Figure 1.A.2 shows that the distribution of *PD estimate*$_{i,b,t-1}$ is right-skewed. Consequently, a 1 standard

Table 1.4.3. Contractionary Monetary Policy Only

Dependent variable:	Δ log Loans$_{i,b,t}$ (1)	Δ Interest rate$_{i,b,t}$ (2)
Rollover need$_{i,b,t}$	-44.00***	-2.00
	(4.16)	(6.35)
Rollover need$_{i,b,t}$ $\times$ MP shock$^{\geq 0}_{t-1}$	-1.93***	4.78***
	(0.51)	(0.90)
Rollover need$_{i,b,t}$ $\times$ MP shock$^{\geq 0}_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	0.13**	-0.18***
	(0.05)	(0.04)
Firm-Bank FE	Yes	Yes
Bank-Time FE	Yes	Yes
Firm-Time FE	Yes	Yes
Remaining interaction terms	Yes	Yes
Adjusted R^2	-0.10	-0.04
R^2	0.51	0.54
Observations	4,027,180	3,852,172

Notes: This table shows empirical estimates of equation (1.4.2) where *MP shock*$_{t-1}$ has been replaced with *MP shock*$^{\geq 0}_{t-1}$. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first column is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in the second is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$^{\geq 0}_{t-1}$ is defined in equation (1.3.2). *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

deviation, i.e. 7.4pp, higher PD attenuates the effect by 42%.[17] Similarly to a lower decrease in the total loan amount, column (8) shows that a monetary policy shock leads to a lower increase in the interest rate for firms with rollover needs with a higher *PD estimate*$_{i,b,t-1}$. A one interquartile range (1 standard deviation) higher PD estimate reduces the impact of the monetary policy shock by 7% (24%). This shows that riskier firms can roll over more of their paid-off loans at better rates. Opposite signs for β_3 for *Loans*$_{i,b,t}$ and *Interest rate*$_{i,b,t}$ support the argument that a credit supply response drives the effect. If it was due to credit demand, both effects should go in the same direction. These results empirically confirm *Hypothesis 2*: Banks reduce credit supply less to risky firms with rollover needs in response to monetary tightening. Note that the negative adjusted R^2 is due to the large number of firm-time fixed effects, as indicated by the large difference from the raw R^2.

The risk-dependent credit supply response of banks is stronger after contractionary monetary policy shocks. Table 1.4.3 shows estimates for equation (1.4.2) where I replace *MP shock*$_{t-1}$ with only contractionary monetary policy shocks, i.e. *MP shock*$^{\geq 0}_{t}$. The estimates for β_3 are substantially larger than in columns (2) and (4) of Table 1.4.2. For *Loans*$_{i,b,t}$ and *Interest rate*$_{i,b,t}$, they increase by a factor of 1.63

17. $2\text{pp} \cdot \frac{0.08}{1.40} = 11\%$ and $7.4\text{pp} \cdot \frac{0.08}{1.40} = 42\%$.

and 1.38, respectively. Hence, the impact of $PD\ estimate_{i,b,t-1}$ is larger after contractionary monetary policy shocks. This confirms *Hypothesis 3*. After contractionary monetary policy shocks, a one standard deviation higher PD estimate attenuates the effect on total loans by 50% instead of 42% and on interest rates by 28% instead of 24% in Table 1.4.2 when using $MP\ shock_{t-1}$. In Appendix section 1.D, I provide a more formal test using quadruple interactions. In Appendix Table 1.D.2, I present results where I estimate equation (1.4.2) with sign-dependent effects, i.e. include contractionary and expansionary monetary policy shocks separately. While the estimates for β_3 with contractionary shocks are similar, the results for expansionary shocks are statistically not significantly different from zero. This supports the idea that banks want to shield firms with rollover needs when an additional reduction in credit supply could be particularly critical for their default, i.e., when they already have a higher default risk. However, this mechanism would not imply that banks increase credit supply less to riskier firms in response to expansionary monetary policy shocks. My results therefore suggest that monetary *tightening* amplifies evergreening incentives for lenders.

Next, I extend the sample to also include single bank firms. To identify the results so far, I exploited variation within firm-time, which restricts the sample to firms that have more than one bank relationship in a given quarter. As many euro area firms rely on one bank relationship only (Kosekova et al., 2023), this yields results only for a part of the whole corporate sector. Therefore, I next adjust equation (1.4.2) and replace firm-time fixed effects $\zeta_{i,t}$ with industry-location-size time fixed effects $\zeta_{ILS(i,t),t}$ (Degryse et al., 2019). This more than doubles the number of observations. However, the identification assumption is now stronger, as I require the loan demand response of firms to monetary policy within industry-location-size cluster in a given quarter not to depend on banks' PD estimate. The median (average) number of unique firms within an industry-location-size-time cluster is 3 (16).

The results identified from multibank firms largely carry over to the sample with single bank firms. Appendix Table 1.A.1 shows the results for estimating equation (1.4.2) with industry-location-size-time instead of firm-time fixed effects. Columns (1) and (3) show results for all monetary policy shocks, while columns (2) and (4) show results for contractionary monetary policy shocks. The estimates for β_1, β_2, and β_3 are similar to earlier estimates in Table 1.4.2 and Table 1.4.3, even though the estimate on β_3 in column (1) is not precisely estimated. In particular, positive estimates for β_3 on total loans and negative estimates on the interest rate show that banks reduce credit supply less to firms with rollover needs after monetary tightening if they have higher default risk.

Results are similar when using the DFR instead of monetary policy shocks. Next, I exchange the measure of monetary policy. Instead of $MP\ shock_{t-1}$, I use changes in the deposit facility rate, ΔDFR_{t-1}. As described above, for this exercise, I also adjust the sample period. As changes in the DFR are autocorrelated, I adjust the regression equation and include the second lag, ΔDFR_{t-2}, fully interacted with $Rollover\ need_{i,b,t}$

and *PD estimate*$_{i,b,t-1}$:

$$
\begin{aligned}
\Delta y_{i,b,t} =& \beta_1^{DFR}\text{Rollover need}_{i,b,t} \\
&+\beta_2^{DFR}\text{Rollover need}_{i,b,t} \times \Delta DFR_{t-1} \\
&+\beta_3^{DFR}\text{Rollover need}_{i,b,t} \times \Delta DFR_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
&+\beta_4^{DFR}\text{Rollover need}_{i,b,t} \times \Delta DFR_{t-2} \\
&+\beta_5^{DFR}\text{Rollover need}_{i,b,t} \times \Delta DFR_{t-2} \times \text{PD estimate}_{i,b,t-1} \\
&+\gamma_1^{DFR}\text{PD estimate}_{i,b,t-1} \\
&+\gamma_2^{DFR}\text{Rollover need}_{i,b,t} \times \text{PD estimate}_{i,b,t-1} \\
&+\gamma_3^{DFR}\Delta DFR_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
&+\gamma_4^{DFR}\Delta DFR_{t-2} \times \text{PD estimate}_{i,b,t-1} \\
&+\eta_{i,b}^{DFR} + \xi_{b,t}^{DFR} + \zeta_{i,t}^{DFR} + \varepsilon_{i,b,t}^{DFR}
\end{aligned}
\tag{1.4.4}
$$

The coefficient of interest remains β_3^{DFR}. Appendix Table 1.A.2 presents the full set of results, where I compare estimates across specifications that include and exclude single-bank firms, as well as using all and only contractionary monetary policy shocks. Across all specifications, the estimated sign and magnitude of β_3^{DFR} indicate that banks reduce credit supply less in response to an increase in the DFR for firms with a higher *PD estimate*$_{i,b,t-1}$. Consistent with my previous findings, the estimated effects for contractionary shocks are larger. Two of the eight estimates are imprecisely estimated and not statistically different from zero, possibly reflecting the fact that changes in the DFR coincide with broader economic developments that may also affect credit supply. In terms of magnitude, the estimates for β_3^{DFR} are about ten to twenty times smaller than those for β_3 in the corresponding regressions, implying that a 10 to 20 basis point change in the DFR is needed to induce an effect comparable to a 1 basis point monetary policy shock. Given that the standard deviation of ΔDFR is about 12.5 times that of the *MP shock*$_{t-1}$, this result is quantitatively reasonable.

1.4.4 Heterogeneity Depending on Banks' Capital Headroom

In this section, I show how the effect varies across banks.

Banks with higher ex-ante capital headroom tend to offer more favorable loan terms to riskier firms with rollover needs following monetary tightening. To test *Hypothesis 4*, I split the sample at the median level of ex-ante capital headroom (6.2 pp.). According to this hypothesis, the coefficient on the interaction term, β_3, should be larger in the subsample of banks with higher capital headroom. I opt for a sample-splitting approach rather than introducing another interaction term to avoid hard-to-interpret quadruple interactions.

Table 1.4.4 presents the results. The dependent variable in the first four columns is $\Delta \log Loans_{i,b,t}$ and $\Delta Interest\ rate_{i,b,t}$ in the remaining columns. Columns (1) and

Table 1.4.4. Heterogeneity Depending on Banks' Capital Headroom

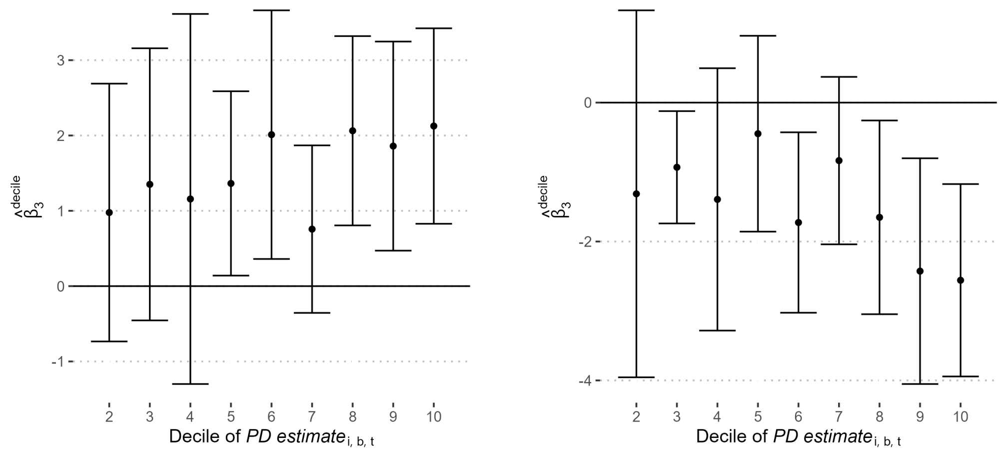
Dependent variable:	Δ log Loans$_{i,b,t}$				Δ Interest rate$_{i,b,t}$			
Monetary policy$_{t-1}$	MP shock$_{t-1}$		MP shock$^{\geq 0}_{t-1}$		MP shock$_{t-1}$		MP shock$^{\geq 0}_{t-1}$	
Capital headroom$_{b,t-1}$	low	high	low	high	low	high	low	high
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollover need$_{i,b,t}$	-60.58***	-29.64***	-58.20***	-28.32**	13.50*	-16.54***	6.78	-23.45***
	(7.47)	(8.14)	(7.37)	(10.41)	(6.47)	(4.33)	(6.92)	(6.51)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$	-1.09**	-1.21	-1.68***	-1.39	4.36***	4.49***	5.40***	6.30**
	(0.48)	(0.97)	(0.37)	(2.02)	(0.76)	(1.11)	(0.55)	(2.36)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	0.06**	0.19***	0.09***	0.26**	-0.07**	-0.16**	-0.09**	-0.24**
	(0.02)	(0.06)	(0.03)	(0.09)	(0.03)	(0.06)	(0.04)	(0.10)
Firm-Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	-0.45	-0.12	-0.45	-0.12	-0.38	-0.13	-0.38	-0.13
R^2	0.54	0.58	0.54	0.58	0.58	0.58	0.58	0.58
Observations	1,427,583	1,476,727	1,427,583	1,476,727	1,310,933	1,464,457	1,310,933	1,464,457

Notes: This table shows empirical estimates of equation (1.4.2). The sample is split according to whether a bank's lagged capital headroom is the lower or upper half of the distribution. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first four columns is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in the remaining columns is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *MP shock*$^{\geq 0}_{t-1}$ is defined in equation (1.3.2). *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. In all columns, the remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

(2), as well as (5) and (6), report estimates for all monetary policy shocks, while the remaining columns focus only on contractionary monetary policy shocks. The odd columns correspond to banks with lower capital headroom, and the even columns correspond to banks with higher capital headroom. In both subsamples, the estimates for β_3 are statistically significant and have the same sign as before; notably, the magnitude of β_3 is about two to three times larger in the high capital headroom subsample than in the low capital headroom subsample. This result confirms *Hypothesis 4*.

This result is consistent with the hypothesized mechanism. Banks with high capital headroom are primarily focused on avoiding defaults among risky firms with rollover needs. In contrast, banks with relatively lower capital headroom face a trade-off: while they seek to avoid defaults, they also benefit from removing high PD, high-risk weight loans from their balance sheets, which improves their capital ratios.

Additional support for this mechanism comes from the differences in the estimates of β_1. The results indicate that, even in the absence of monetary policy shocks, a firm with an average probability of default (PD) estimate experiences a more pronounced reduction in total loans - and a larger increase in interest rates - when it has rollover needs when borrowing from a bank with lower capital headroom. Al-



(a) Δ log Loans

(b) Δ Interest rate

Figure 1.4.1. Non-Linear Impact of PD Estimate

Notes: This figure visualizes empirical estimates from a version of equation (1.4.1) in which *PD estimate*$_{i,b,t-1}$ is replaced with dummies for the different deciles. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the left figure is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in the right figure is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. The omitted category is the lowest decile. Bars depict 95% confidence intervals.

though β_1 is not identified in this setting, the observed pattern is consistent with the abovementioned mechanism.

1.4.5 Non-Linear Impact of the Default Risk

In this section, I provide evidence for a non-linear impact of the PD.

In the regression so far, I estimated a linear impact of *PD estimate*$_{i,b,t-1}$ on banks' credit supply response. Faria-e-Castro, Paul, and Sánchez (2024) stress how lenders' incentives to provide more favorable loan terms to riskier lenders might be particularly pronounced for firms close to default. This would suggest a non-linear impact of firms' default risk. To address this empirically, I split *PD estimate*$_{i,b,t-1}$ in ten deciles. I then rerun equation (1.4.2) and replace *PD estimate*$_{i,b,t-1}$ with dummies for the ten deciles.

The effect is most pronounced for firms in the highest decile of PDs, supporting the idea that banks prioritize shielding particularly vulnerable firms from tighter loan terms. Figure 1.4.1 presents the results for estimates on β_3 for the different PD deciles. The left figure shows results where the dependent variable is $\Delta \log Loans_{i,b,t}$ and the right figure where the dependent variable is Δ *Interest rate*$_{i,b,t}$. The omitted category is the lowest decile. The figures reveal the expected non-linearity: While the impact of monetary policy on banks' credit supply reduction to firms with rollover needs is mostly not significantly different within the five lowest deciles, there is a stronger effect in the lower deciles. The effect is strongest for the highest decile of *PD estimate*$_{i,b,t-1}$, exactly in line with the empirical results in Faria-e-Castro, Paul, and Sánchez (2024). This supports the idea that when banks adjust their credit supply to firms with rollover needs to changes in monetary policy, they shield only those firms from tighter loan terms for which these loan terms might be critical to default, i.e., firms that were ex-ante already notably riskier.

To see this non-linearity empirically, focus on the comparison between the lowest and the highest decile. The difference in the average *PD estimate*$_{i,b,t-1}$ between the observations in these two deciles is 18.7 percentage points. Based on the results for β_3 in columns (2) and (4) of Table 1.4.2, this would imply a 1.5 pp. higher growth rate of *Loans*$_{i,b,t}$ and a 2.4 bps lower increase of *Interest rate*$_{i,b,t}$. However, the effects in Figure 1.4.1 are 2.13 and 2.6. Thus, in particular, the effect on the loan amount is notably higher.

I explore this non-linear relationship further by estimating variants of the results above. Instead of *PD estimate*$_{i,b,t-1}$, I use a dummy variable that equals one if the PD estimate is within the top decile of PD estimates. I denote this dummy variable by

$$\text{High PD estimate}_{i,b,t} := \begin{cases} 1 & \text{if PD estimate}_{i,b,t} \geq P_{90}(\text{PD estimate}_{i,b,t}) \\ 0 & \text{else} \end{cases} \tag{1.4.5}$$

where $P_{90}(\text{PD estimate}_{i,b,t})$ denotes the 90th percentile. Table 1.4.5 shows the results: In this table, I show evidence for the sample of multibank firms as well as all firms and all as well as contractionary monetary policy shock. The cutoff for the highest decile is 6.1% in the sample of multibank firms and 7.1% in the other sample. Throughout specifications, a bank reduces credit supply less in response to monetary tightening to firms with rollover needs when the bank's estimate for the probability of default is within the top decile of PD estimates, i.e., when the firm is close to default. Again, estimates are larger for contractionary shocks. I use this clear result to explore alternative explanations below. In Appendix Table 1.A.3, I provide robustness by showing that results are similar when picking the highest quintile instead of the highest decile.

Table 1.4.5. Non-linear impact of the PD estimate

Dependent variable:	Δ log Loans$_{i,b,t}$				Δ Interest rate$_{i,b,t}$			
Single bank firms included:	No		Yes		No		Yes	
Monetary policy$_{t-1}$:	MP shock$_{t-1}$	MP shock$_{t-1}^{\geq 0}$	MP shock$_{t-1}$	MP shock$_{t-1}^{\geq 0}$	MP shock$_{t-1}$	MP shock$_{t-1}^{\geq 0}$	MP shock$_{t-1}$	MP shock$_{t-1}^{\geq 0}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollover need$_{i,b,t}$	-46.25***	-44.29***	-44.25***	-41.74***	4.96	0.12	-2.85	-9.02
	(3.73)	(3.92)	(6.07)	(6.45)	(4.95)	(6.12)	(6.33)	(7.75)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$	-1.25***	-1.68***	-1.36**	-2.01**	3.75***	4.42***	4.64***	5.55***
	(0.33)	(0.44)	(0.58)	(0.70)	(0.67)	(0.86)	(0.77)	(1.01)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$ $\times$ High PD estimate$_{i,b,t}$	0.80**	0.95**	1.15**	1.79***	-1.28**	-1.67*	-1.64	-2.87**
	(0.27)	(0.40)	(0.44)	(0.55)	(0.54)	(0.77)	(0.92)	(0.96)
Firm-Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry-Location-Size-Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P_{90}(PD estimate$_{i,b,t}$)	6.1%	6.1%	7.1%	7.1%	6.1%	6.1%	7.1%	7.1%
Adjusted R^2	-0.09	-0.09	0.07	0.07	-0.04	-0.04	0.07	0.07
R^2	0.51	0.51	0.20	0.20	0.53	0.53	0.21	0.20
Observations	4,006,751	4,006,751	9,206,264	9,206,264	3,833,287	3,833,287	8,936,324	8,936,324

Notes: This table shows empirical estimates of variants of equation (1.4.2). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans in columns (1), (2), (5), and (6). In the remaining columns, single-bank firms are also included. The dependent variable in columns (1) to (4) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (5) to (8) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). The monetary policy measure in columns (1), (3), (5), and (7) is *MP shock*$_{t-1}$,which is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. In the remaining columns, the monetary policy measure is *MP shock*$_{t-1}^{\geq 0}$, which is defined in equation (1.3.2). *High PD estimate*$_{i,b,t-1}$ is a dummy variable defined in equation (1.4.5) that is 1 if the bank's individual lagged estimate of the firm's risk to default within the next year is within the top decile of the distribution. Industry is the 2digit NACE code. The location is the NUTS3 region. Size is a categorical variable for large, medium, small, and micro enterprises according to the Annex to Commission Recommendation 2003/361/EC. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

1.5 Aggregate Relevance

This section introduces two exercises that shed light on the aggregate relevance of banks' muted credit supply response to monetary tightening for riskier firms with potential rollover needs.

1.5.1 Relevance of Loans Used to Roll Over Paid-Off Loans

The bank lending channel posits that monetary policy affects banks' supply of new loan contracts. Firms can use new loan contracts to extend their debt level, replace old loans early (if loan contracts permit), partly pay off loans (if loan contracts permit), or roll over maturing loans. Figure 1.5.1 shows the share of the volume of all new loan contracts that is used to roll over maturing loans. This share is potentially prone to the evergreening behavior of banks I described above. On average, about 42% of the volume of new loan contracts is used to roll over loans that are paid off at maturity. Therefore, a significant share of new lending business happens when firms roll over existing debt and is consequently affected by the evergreening mechanism I describe in this paper. Admittedly, this simple exercise ignores that this ratio is already an equilibrium outcome and might be lower if banks did not relatively increase credit supply to risky firms with potential rollover needs.

1.5.2 Effect of Bank Behavior on Monetary Policy-Induced Financing Gap

In this section, I use the local effects estimated above to show that banks' tendency to reduce credit supply less in response to monetary tightening to firms with potential rollover needs that are closer to default substantially lowers the financing gap induced by monetary policy.

Monetary tightening leads to a financing gap for firms that pay off maturing loans relative to a situation where monetary policy remains unchanged. As shown above, this financing gap stems from two sources: First, as less attractive loan terms induce the firm to roll over a smaller share of its paid-off debt, it has to use more of its current liquidity to pay off the debt that is not rolled over. Second, as the interest rate on the remaining debt increases, the firm's interest expenses increase. In order to service this, it has to cut other expenses. The sum of these two effects is the total financing gap induced by monetary policy through firms that pay off loans.

To illustrate the importance of the bank's relative credit supply increase to riskier firms when monetary policy tightens, I compare the financing gap induced by a 10bps monetary policy shock for safe and risky firms in two back-of-the-envelope scenarios. In one scenario, I use my empirical estimates of equation (1.4.2). This specification reflects evergreening behavior, i.e., that banks increase credit supply to riskier firms. In a counterfactual scenario, I impose $\beta_3 = 0$ and leave the remaining coefficients unchanged, i.e., I shut off banks' risk-dependent credit supply adjustment to monetary policy. The comparison of the monetary policy-induced financing

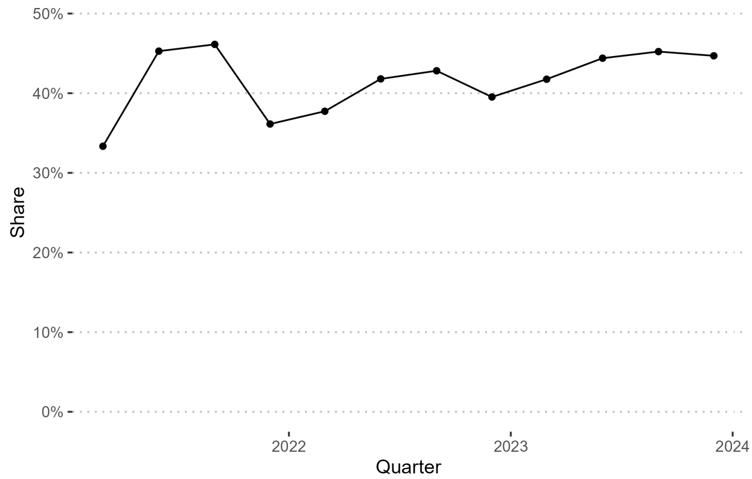


Figure 1.5.1. Share of Aggregate New Loan Volume Used for Rollovers

Notes: This figure shows the share of aggregate new loans used for rollover in total new loans. $Rollover_{i,b,t}$ is defined as min(new loans$_{i,b,t}$, paid off loans at maturity$_{i,b,t}$). *Paid off loans at maturity*$_{i,b,t}$ denote the volume of all loans borrowed by firm i from bank b that were reported in $t-1$ for the last time and, thus, paid off in quarter t. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans.

gap between these two scenarios illustrates the importance of banks' behavior. Of course, this back-of-the-envelope calculation is based on local effects and ignores potential general equilibrium adjustments.

In addition to $MP\ shock_{t-1} = 10$, I fix $Rollover\ need_{i,b,t} = 0.02$, i.e. at its average value. Furthermore, for illustration purposes, I fix $Loans_{i,b,t-1} = 100$, but the effect is linear in the total loan amount. I focus on the financing gap in one quarter. In Appendix section 1.E, I describe in detail how I calculate the total financing gap in the two scenarios.

Figure 1.5.2 plots the financing gaps for different scenarios, given a 10bps monetary policy shock in one quarter. On the x-axis are different levels of $PD\ estimate_{i,b,t-1}$ and on the y-axis the financing gap in cents. The bars with stripes show financing gaps in a counterfactual scenario where I impose $\beta_3 = 0$. In contrast, the bars with dots use $\beta_3 = \hat{\beta}_3$ from columns (2) and (4) of Table 1.4.2. I take all other estimates these columns in both scenarios. The difference between the bars shows how much a firm with the associated PD benefits from banks' evergreening behavior following monetary tightening.

For a firm with an average (median) PD of 3.1% (1.0%), switching on β_3 reduces the financing gap by 7 cents from 47 to 40 (3 cents from 48 to 45) or 15% (6%). To put this magnitude into perspective, imagine a firm that does not roll over or repay any loans and simply keeps its outstanding debt level constant. How much would its average interest rate on that debt have to change to get the same financing gap?

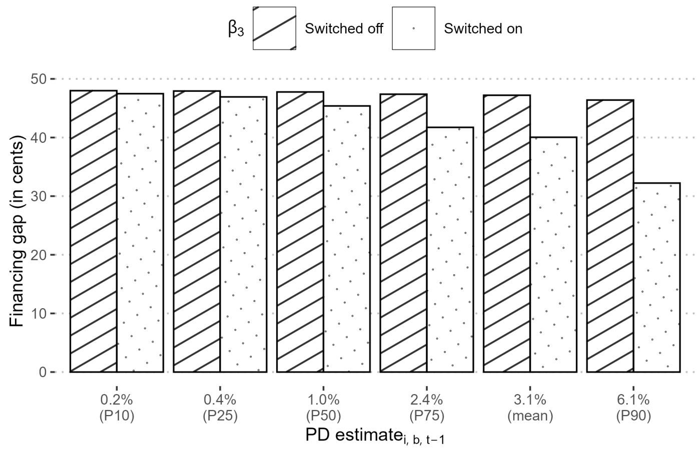


Figure 1.5.2. Effect of Banks' Selective Credit Supply Adjustment on Firms' Financing Gap Along the PD Distribution

Notes: This figure shows results for a back-of-the-envelope calculation on the financing gap from a 10 bps monetary policy shock on a firm with an average potential rollover need of 0.02 and a loan amount of €100, depending on the PD estimate and whether β_3 is switched off or on. Appendix section 1.E gives details of the calculation.

For a firm with € 100, the annualized interest rate would have to increase by 28bps (12bps) to have a similar effect.

Ippolito, Ozdagli, and Perez-Orive (2018) estimate for the US, that the *annual* effect of a 1pp increase in the FED funds rate through the floating-rate channel is between \$0.32 and \$0.44 on a \$100 loan and not more than \$0.3 through the bank lending channel. This shows that the magnitude of my channel is relevant, as the annual effect would be 28 cents for the average firm.

Importantly, the effect of switching on β_3 increases with the PD. While for firms with a low PD estimate, the effect that banks increase credit supply in response to monetary tightening for riskier firms with rollover needs intuitively hardly matters, it saves a firm at the 90th percentile of the PD distribution about 14 cents per quarter, as it reduces the financing gap from 46 to 32 cents. This is a reduction of 30%. To put this magnitude into perspective: To save 14 cents on a € 100 loan in a quarter without any rollover happening, the interest rate of the firm would have to decrease by 56 bps, which is more than one standard deviation of the quarter-on-quarter change of interest rates in my sample. As shown above, the effects of contractionary monetary policy are even larger.

Similar to what Ippolito, Ozdagli, and Perez-Orive (2018) note about the floating-rate channel, the effect through potential rollover needs affects firms' *internal* financing gap. This is in contrast to the bank lending channel, which affects *external* financing. Through its impact on equity, internal financing gaps can further amplify monetary policy transmission, e.g., via the collateral or balance sheet channel.

1.6 Alternative Explanations

In this section, I explore alternative explanations that would be consistent with the empirical results presented above. I provide evidence that shows how these explanations do not drive my results. First, I show that results are not driven by differences across banks in the riskiness of their corporate lending portfolio. Second, I demonstrate that results are robust when studying aggregate instead of bank-specific rollover needs. Finally, I conduct several further robustness checks.

1.6.1 Differences Across the Riskiness of Banks' Corporate Lending Portfolios

I argue that banks reduce credit supply to firms with rollover needs less in response to tighter monetary policy when they estimate these firms to have higher default risk, specifically due to the consequences of this *individual* default risk. However, the triple interaction results presented above - e.g., in Table 1.4.2 - would also be consistent with an alternative mechanism based on differences in the aggregate riskiness of banks' corporate lending portfolios. To illustrate this alternative mechanism, consider a setting with two types of banks, A and B, each lending exclusively to firms with either a low or high $\textit{PD estimate}_{i,b,t-1}$. Specifically, assume there are only two possible values for PD estimate, and that type A banks lend only to firms with the low PD value, while type B banks lend only to firms with the high PD value. In this case, one could replace $\textit{PD estimate}_{i,b,t-1}$ in equation (1.4.2) with a dummy variable indicating bank type, $\textit{Bank type}_b$, because there is a one-to-one relationship between the PD and the bank type. While the estimate for this dummy itself as well as for its interaction with monetary policy would be absorbed by bank-time fixed effects, the coefficient β_3 on $\textit{Rollover need}_{i,b,t} \times \textit{MP shock}_{t-1} \times \textit{Bank type}_b$ would still be estimated from variation at the i, b, t level.

Now, suppose banks with riskier portfolios (type B) respond differently to monetary policy shocks - for instance, by reducing their overall credit supply less in response to tightening, irrespective of individual borrower risk. This would also generate a negative estimate for β_3 on $\textit{Loans}_{i,b,t}$ and a positive estimate for $\textit{Interest rate}_{i,b,t}$. In this scenario, banks would reduce credit supply to firms with rollover needs less not because of the borrower's *individual* default risk but due to the *aggregate* riskiness of their portfolio. Column (5) of Table 1.4.1 shows that bank-time fixed effects explain about 18% of the variation in PD estimates, indicating indeed some cross-bank differences in aggregate portfolio risk. To isolate the role of individual default risk, I present evidence that explicitly exploits within bank-time variation in PD estimates.[18]

First, I replace $\textit{PD estimate}_{i,b,t-1}$ with its deviation from the bank-time specific mean:

18. This approach is similar to Ottonello and Winberry (2020, p. 2478).

Table 1.6.1. Effect of Variation in PD Estimates Within Bank

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
PD estimate measure$_{i,b,t-1}$	$\widetilde{\text{PD estimate}}_{i,b,t-1}$	High $\widetilde{\text{PD estimate}}_{i,b,t}$	$\widetilde{\text{PD estimate}}_{i,b,t-1}$	High $\widetilde{\text{PD estimate}}_{i,b,t}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-46.18***	-47.50***	4.82	5.70
	(3.69)	(3.85)	(4.99)	(4.98)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$	-1.21***	-1.30***	3.65***	3.80***
	(0.33)	(0.33)	(0.65)	(0.67)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ PD estimate measure$_{i,b,t-1}$	0.10**	0.92*	-0.08**	-1.23**
	(0.04)	(0.50)	(0.03)	(0.55)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes
Adjusted R^2	-0.09	-0.09	-0.04	-0.04
R^2	0.51	0.51	0.53	0.53
Observations	4,006,751	4,006,751	3,833,287	3,833,287

Notes: This table shows empirical estimates of variants of equation (1.4.2) with different PD estimate measures. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in columns (1) to (2) is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in columns (3) to (4) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. The PD estimate measure in columns (1) and (2) is the deviation of *PD estimate*$_{i,b,t-1}$ from the time-varying bank-specific average as defined in equation (1.6.1), measured in percentage points. In columns (3) and (4), it is a dummy variable that equals 1 if the PD estimate is in the top decile of the time-varying bank-specific PD distribution, as defined in equation (1.6.2). The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

$$\begin{aligned}\widetilde{\text{PD estimate}}_{i,b,t-1} &:= \text{PD estimate}_{i,b,t-1} - \overline{\text{PD}}_{b,t-1} \\ &= \text{PD estimate}_{i,b,t-1} - \frac{\sum_i \text{PD estimate}_{i,b,t} \cdot \mathbb{1}(\text{Loans}_{i,b,t-1} > 0)}{\sum_i \mathbb{1}(\text{Loans}_{i,b,t-1} > 0)}\end{aligned} \tag{1.6.1}$$

Here, $\widetilde{\text{PD estimate}}_{i,b,t-1}$ captures the deviation of a bank's estimated default risk for an individual firm from the bank's aggregate estimate. Even if banks differ in their overall portfolio risk and adjust their credit supply accordingly, this variation is absorbed by $\overline{\text{PD}}_{b,t-1}$.

Second, I revisit the non-linearity analysis. Instead of defining high PD firms based on the upper decile of the full sample distribution, I create a dummy that indicates firms in the top decile of their *bank-time specific* PD distribution: Adjusting equation (1.4.5), I define

$$\text{High } \widetilde{\text{PD estimate}}_{i,b,t} := \begin{cases} 1 & \text{if PD estimate}_{i,b,t} \geq P_{90}(\text{PD estimate}_{i,b,t} | b, t) \\ 0 & \text{else} \end{cases} \tag{1.6.2}$$

where $P_{90}(\text{PD estimate}_{i,b,t}|b,t)$ denotes the 90th percentile of the bank-time specific PD distribution.

Table 1.6.1 presents the results. Columns (1) and (3) use $\widetilde{\text{PD estimate}}_{i,b,t-1}$ from equation (1.6.1), while columns (2) and (4) use High PD estimate$_{i,b,t}$ from equation (1.6.2). Compared to Tables 1.4.2 and 1.4.5, estimates for β_3 remain similar in magnitude, with slightly larger effects on $Loans_{i,b,t}$ and somewhat smaller effects on *Interest rate*$_{i,b,t}$.

Overall, these results confirm that my findings are not driven by differences in banks' aggregate portfolio risk. By leveraging within bank-time variation in PD estimates, I demonstrate that the observed effects stem from banks responding to the *individual* default risk of borrowers rather than the *aggregate* riskiness of their loan portfolios.

1.6.2 Firms Switching Between Banks

In this section, I show that my results also hold when I use the rollover need aggregated at the firm-time level.

A second concern about my findings could relate to my measure of *Rollover need*$_{i,b,t}$. In equation (1.3.1), I defined this measure to vary between banks and firms. That is, I imposed a bank-specific rollover need. Thus, this measure assumes that the rollover of maturing debt happens within bank-firm relations. However, firms could also switch between banks to roll over maturing debt. In this section, I use an alternative measure of rollover need. I do so by aggregating the rollover need of a firm in a given quarter across all banks. I introduced this measure in the section on identification and defined it in equation (1.4.3). Then I estimate

$$\begin{aligned}\Delta y_{i,b,t} =& \phi_1 \text{High PD estimate}_{i,b,t-1} \\ &+\phi_2 \text{High PD estimate}_{i,b,t-1} \times \text{MP shock}_{t-1} \\ &+\phi_3 \text{High PD estimate}_{i,b,t-1} \times \text{MP shock}_{t-1} \times \text{Rollover need}_{i,t} \\ &+\phi_4 \text{High PD estimate}_{i,b,t-1} \times \text{Rollover need}_{i,t} \\ &+\eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t}\end{aligned} \tag{1.6.3}$$

where the main variable of interest is ϕ_3. Since *Rollover need*$_{i,t}\times$*MP shock*$_{t-1}$ does not vary within firm-time buckets, ϕ_3 cleanly identifies the differential impact on $\Delta y_{i,b,t}$ for a firm with a higher aggregate rollover need after a monetary policy shock that has a *High PD estimate*$_{i,b,t-1} = 1$. The drawback of this specification is that it does not rule out the possibility that banks increase credit supply to riskier firms in response to monetary tightening, even though these firms have no maturing debt with these banks.

Table 1.6.2 shows results of estimating variants of equation (1.6.3). I use two different measures of default risk, namely the dummy for *PD estimate*$_{i,b,t-1}$ in the

Table 1.6.2. Rollover Need Aggregated at the Firm-Time Level

Dependent variable:	Δ log Loans$_{i,b,t}$				Δ Interest rate$_{i,b,t}$			
PD estimate measure$_{i,b,t-1}$	High PD estimate$_{i,b,t}$		High PD $\widetilde{\text{estimate}}_{i,b,t}$		High PD estimate$_{i,b,t}$		High PD $\widetilde{\text{estimate}}_{i,b,t}$	
Monetary policy$_{t-1}$:	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PD estimate measure$_{i,b,t-1}$	-0.41**	-0.45**	-0.32	-0.37	-0.73*	-0.92**	-0.57	-0.74*
	(0.16)	(0.19)	(0.19)	(0.22)	(0.34)	(0.34)	(0.36)	(0.37)
PD estimate measure$_{i,b,t-1}$ × Monetary policy$_{t-1}$	0.04	0.04	0.02	0.03	0.10*	0.15***	0.06	0.11**
	(0.03)	(0.04)	(0.02)	(0.03)	(0.05)	(0.04)	(0.06)	(0.04)
PD estimate measure$_{i,b,t-1}$ × Monetary policy$_{t-1}$ × Rollover need$_{i,t}$	1.25**	1.97***	1.71***	1.98***	-2.09**	-2.63*	-1.46*	-2.07**
	(0.43)	(0.43)	(0.26)	(0.45)	(0.89)	(1.27)	(0.68)	(0.81)
PD estimate measure$_{i,b,t-1}$ × Rollover need$_{i,t}$	-11.24***	-13.67***	-8.68**	-10.60**	18.20**	21.16***	9.34*	12.04**
	(2.62)	(2.92)	(3.38)	(4.45)	(5.96)	(6.27)	(4.40)	(4.62)
Firm-Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	-0.10	-0.10	-0.10	-0.10	-0.04	-0.04	-0.04	-0.04
R^2	0.50	0.50	0.50	0.50	0.53	0.53	0.53	0.53
Observations	4,006,751	4,006,751	4,006,751	4,006,751	3,833,287	3,833,287	3,833,287	3,833,287

Notes: This table shows empirical estimates of variants of equation (1.6.3). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in columns (1) to (4) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (5) to (8) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. The PD estimate measure in columns (1), (2), (5), and (6) is *High PD estimate*$_{i,b,t-1}$, a dummy variable defined in equation (1.4.5) that is 1 if the bank's individual lagged estimate of the firm's risk to default within the next year is within the top decile of the distribution. In the remaining columns, it is a dummy variable that equals 1 if the PD estimate is in the top decile of the time-varying bank-specific PD distribution, as defined in equation (1.6.2). The monetary policy measure in columns (1), (3), (5), and (7) is *MP shock*$_{t-1}$, which is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. In the remaining columns, the monetary policy measure is *MP shock*$^{\geq 0}_{t-1}$, which is defined in equation (1.3.2). Standard errors are clustered at the firm-bank and quarter level.

top decile of the whole distribution and the dummy when this estimate is in the top decile of the bank-time specific distribution. I show results for all and contractionary monetary policy shocks separately. The estimates of ϕ_1 on the effect of the *PD estimate measure*$_{i,b,t-1}$ are inconclusive concerning a credit supply effect, as estimates have the same sign for both dependent variables. The estimates for ϕ_2 are also inconclusive, but the positive significant estimates on *Interest rate*$_{i,b,t}$ for *Rollover need*$_{i,b,t}$=0 would be in line with the traditional risk-taking channel of monetary policy.

As my sample only includes firm-bank relationships where there was at least some debt outstanding in the previous quarter, some evergreening incentives are present even though the *Rollover need* is now aggregated. Therefore, estimates for ϕ_3 still have a meaningful interpretation. Indeed, empirical estimates confirm the findings above: Estimates are positive and significant for *Loans* and negative and significant for *Interest rate*. That is, banks reduce credit supply less in response to a monetary policy shock to firms with rollover needs if they estimate the firm to be more vulnerable. As before, estimates are larger for contractionary monetary policy shocks. The variation to estimate ϕ_3 now comes only from variation across banks in a given quarter whether their PD estimate is in the top decile of the overall or bank-specific PD distribution or not, as *Rollover need*$_{i,b,t}$ × *Monetary policy*$_{t-1}$ is absorbed by fixed effects.

The average *Rollover need*$_{i,t}$ aggregated at the firm-time level is 2%, similar to the *Rollover need*$_{i,b,t}$ at the firm-bank-time level. Therefore, for example, estimates from columns (3) and (7) can be compared with estimates from columns (2) and (4) in Table 1.6.1. This comparison shows that the magnitude of the estimates for both dependent variables is larger. This shows that the mechanism is also present when considering the aggregated firm rollover need.

How often do different banks differ in whether they estimate the same borrower to have a PD in the top decile of the distribution or not at a given time? One might expect that banks would differ only slightly in their estimates for such high PD borrowers, but not enough to place many firms in different deciles. However, I document substantial variation. First, regressing *High PD estimate*$_{i,b,t-1}$ on firm-bank, bank-time, and firm-time fixed effects jointly leaves around 20% in the variation of this dummy variable unexplained. Second, out of all firm-bank-time observations with *High PD estimate*$_{i,b,t-1} = 1$, for about 73% there exists at least one other observation with the same firm in the same quarter with *High PD estimate*$_{i,b,t-1} = 0$. Third, Appendix Figure 1.A.3 shows that the distance between the 90th percentile and the *PD estimate*$_{i,b,t-1}$ of firm-bank-time observation with *High PD estimate*$_{i,b,t-1} = 0$ but where at least one bank estimates the *PD estimate*$_{i,b,t-1}$ of the firm in that quarter to be in the top decile has a high variation, implying that the variation in *High PD estimate*$_{i,b,t-1}$ does not only come from firms being just above and just below the threshold. Put differently, model variation across banks is large, as documented by Berg and Koziol (2017).

Table 1.6.3. Comparison of Two Measures for Rollover Need

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
PD estimate measure$_{i,b,t-1}$	High PD estimate$_{i,b,t-1}$ (1)	$\widetilde{\text{High PD estimate}}_{i,b,t-1}$ (2)	High PD estimate$_{i,b,t-1}$ (3)	$\widetilde{\text{High PD estimate}}_{i,b,t-1}$ (4)
Rollover need$_{i,b,t}$	-48.75***	-49.99***	11.71*	12.51*
	(4.00)	(4.12)	(6.03)	(6.11)
Rollover need$_{i,b,t}$ $\times$ PD estimate measure$_{i,b,t-1}$	8.01***	13.09***	-10.86*	-12.82**
	(2.13)	(3.27)	(5.46)	(4.50)
Rollover need$_{i,t}$ $\times$ PD estimate measure$_{i,b,t-1}$	-18.02***	-12.06**	23.79***	16.88***
	(3.47)	(3.90)	(4.42)	(3.46)
PD estimate measure$_{i,b,t-1}$	-0.31*	-0.42**	-0.56	-0.42
	(0.16)	(0.18)	(0.32)	(0.31)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Adjusted R^2	-0.09	-0.09	-0.04	-0.04
R^2	0.51	0.51	0.53	0.53
Observations	4,006,751	4,006,751	3,833,287	3,833,287

Notes: This table show empirical estimates of variants of $\Delta y_{i,b,t} = \pi_1 \text{Rollover need}_{i,b,t} + \pi_2 \text{Rollover need}_{i,b,t} \times \text{PD estimate measure}_{i,b,t-1} + \pi_3 \text{Rollover need}_{i,t} \times \text{PD estimate measure}_{i,b,t-1} + \pi_4 \text{PD estimate measure}_{i,b,t-1} + \eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t}$. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in columns (1) to (2) is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in columns (3) to (4) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. The PD estimate measure in columns (1) and (3) is *High PD estimate*$_{i,b,t-1}$, a dummy variable defined in equation (1.4.5) that is 1 if the bank's individual lagged estimate of the firm's risk to default within the next year is within the top decile of the distribution. In columns (2) and (4), it is a dummy variable that equals 1 if the PD estimate is in the top decile of the time-varying bank-specific PD distribution, as defined in equation (1.6.2). Standard errors are clustered at the firm-bank and quarter level.

The estimates for ϕ_4 on the interaction of the PD estimate measure with *Rollover need*$_{i,t}$ illustrate a drawback of this alternative measure of rollover need. The negative estimates for *Loans* and the positive ones for *Interest rate* indicate that banks reduce credit supply more to firms they estimate to be riskier when these firms have rollover needs with any bank. This is not surprising, as banks, in general, should offer less favorable loan terms to firms they estimate to be riskier. Imagine a firm that has loans maturing with bank *A* and approaches banks *B* and *C* for new loan contracts that it wants to use to roll over the maturing loans. If bank *B* estimates the firm to be riskier than bank *C* does, this will show up as a reduction of credit supply by bank *B* to firms with rollover needs. Put differently, with the rollover need aggregate at the firm-time level *i,t*, it is no longer possible to see whether there are evergreening motives involved. This view is supported by results shown in Table 1.6.3 which compares the effect of the *Rollover need* at the firm-bank-time level i, b, t to that aggregated at the firm-time level i, t . The estimates in the third row on the rollover need at the bank-firm-time level show that a bank increases credit supply more to firms for which it estimates a high default risk when these firms have rollover needs with this very bank, i.e., *Rollover need*$_{i,b,t}$ $>$ *Rollover need*$_{i,t} > 0$. In contrast, when the firm has rollover needs with other banks, i.e., *Rollover need*$_{i,b,t} = 0$ and *Rollover need*$_{i,t} > 0$, the bank reduces exposure to this risky firm *relative to other banks*.

1.6.3 Further Robustness Checks

Maturity. Fabiani, Heineken, and Falasconi (2024) show that monetary policy affects the maturity structure of corporate debt in that a monetary tightening shortens corporate debt maturity. If this effect was stronger for riskier firms and shorter maturities were associated with lower interest rates, this could explain the part of my results where the dependent variable is *Interest rate*. In this case, banks would not relatively increase credit supply to riskier firms in response to monetary policy but disproportionally reduce the maturity of the new loans used to roll over maturing debt. To test this, I estimate equation (1.4.2) with $\Delta y_{i,b,t} = \Delta \log(\text{Maturity})_{i,b,t}$. The results in Appendix Table 1.A.4 show that paying off loans intuitively increases maturity, as loans close to maturity with naturally very low maturity are either paid off or replaced by loans with longer maturity. On average, when a firm pays off all its loans, maturity increases by 218%. However, the estimate on β_3 is insignificant. The maturity of firms that have rollover needs after a monetary tightening does not react differently for firms for which banks estimate a higher PD. Therefore, my results are not driven by riskier firms getting loans with a different maturity

Measuring Rollover Need. The results are robust to alternative definitions of rollover need. The measure introduced in equation (1.3.1) requires choices, but alternative variations yield similar results. Appendix Table 1.A.5 reports robustness tests. In columns (1) and (2), I define rollover need based on loans paid off in the quarter before maturity rather than the full year before maturity. In columns (3) and (4), I consider all loans reaching the quarter before maturity, regardless of whether they were paid off or not. The estimates for β_3 are economically and statistically significant, positive for the loan amount, and negative for the interest rate.

Systematically Biased PD Estimates. I address two concerns about potential biases in $PD\ estimate_{i,b,t-1}$ that could bias my results. First, my results are not driven by low-capital banks estimating lower *PD*s. Previous studies uncover drivers of the heterogeneity in PD estimates. Plosser and Santos (2018) show that banks with less capital report lower PDs and Behn, Haselmann, and Vig (2022) find that banks that are more capital-constrained and banks for which the loan book accounts for a larger share of total assets estimate lower PDs. This could raise the concern that my results show that monetary policy tightening leads to lower credit supply to firms with rollover need borrowing from high-capital banks rather than firms with higher default probabilities. That would be consistent with Jiménez, Ongena, Peydró, and Saurina (2012), who find that banks react more strongly to monetary policy when they have low capital or liquidity. However, such differences at the bank-time level are tackled by $\overline{\mathrm{PD}}_{b,t-1}$ as discussed above in Section 1.6.1 where I document that the result also holds for differences in PD estimates within bank.

Second, my results are not driven by banks estimating lower PDs for firms that are more important to them. The robustness check described above addresses concerns about PDs varying systematically across banks over time. If the PD estimates differed systematically across firms within bank-quarter buckets, this could impede my identification strategy. Firestone and Rezende (2016) find that "banks assign lower PDs to loans of which they hold larger shares, suggesting that incentives affect risk parameters." This would imply that banks reduce credit supply less in response to monetary tightening to firms that are of less importance to them instead of firms with higher default risk. To address these concerns, I modify equation (1.4.2) and add the share of lending towards a firm in a bank's total lending,

$$\frac{\mathrm{Loans}_{i,b,t-1}}{\sum_i \mathrm{Loans}_{i,t-1}},$$

which measures the importance of lending to the firm for the bank. I fully interact this share with all other terms and estimate the following regression equation:

$$
\begin{aligned}
\Delta y_{i,b,t} = & \beta_1 \text{Rollover need}_{i,b,t} \\
& + \beta_2 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \\
& + \beta_3 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \beta_4 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \frac{\text{Loans}_{i,b,t-1}}{\sum_i \text{Loans}_{i,t-1}} \\
& + \gamma_1 \text{PD estimate}_{i,b,t-1} \\
& + \gamma_2 \text{Rollover need}_{i,b,t} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_3 \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_4 \frac{\text{Loans}_{i,b,t-1}}{\sum_i \text{Loans}_{i,t-1}} \\
& + \gamma_5 \text{Rollover need}_{i,b,t} \times \frac{\text{Loans}_{i,b,t-1}}{\sum_i \text{Loans}_{i,t-1}} \\
& + \gamma_6 \text{MP shock}_{t-1} \times \frac{\text{Loans}_{i,b,t-1}}{\sum_i \text{Loans}_{i,t-1}} \\
& + \eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t}
\end{aligned} \tag{1.6.4}
$$

Appendix Table 1.A.6 shows the results, where estimates for β_3 hardly differ from those in Table 1.4.2 after adding the control variable.

Potential Impact of Monetary Policy on PD Estimates. The *PD estimate*$_{i,b,t-1}$ is reported at the end of the quarter in which the monetary policy shocks occur. While regulatory PD estimates are designed to be "through-the-cycle" measures based on long-term averages, it remains possible that monetary policy shocks influence them.[19] For instance, if a contractionary monetary policy shock leads to a higher *PD estimate*$_{i,b,t-1}$ for firms with rollover needs in the following quarter, the estimate for β_3 could be biased. To address this concern, Appendix Table 1.A.7 presents results using the PD estimate lagged by two quarters, *PD estimate*$_{i,b,t-2}$. The estimates for β_3 remain statistically significant and similar in magnitude to previous results, confirming that the findings are not driven by monetary policy shocks affecting PD estimates.

Raw Monetary Policy Surprises. Given the recent debate questioning the validity of information purging (Bauer and Swanson, 2023a,b), I assess robustness using raw monetary policy surprises. Appendix Table 1.A.8 presents the results. While the signs of the estimates for β_3 remain unchanged, their magnitudes are roughly half as large. This aligns with the fact that the standard deviation of the raw monetary policy surprise measure is approximately twice that of *MP shock*$_{t-1}$. Notably, only

19. For details on the requirements for PD estimates, see European Parliament and Council of the European Union (2013, Article 180).

the estimates for contractionary shocks are precisely identified, highlighting once again the importance of studying asymmetric effects.

1.7 Conclusion

In this paper, I show how monetary tightening induces evergreening behavior. In doing so, I present new evidence on the effect of borrower risk in determining lenders' adjustment of credit supply to monetary policy. According to the traditional risk-taking channel, lenders reduce the supply of new credit to riskier borrowers more when monetary policy tightens. For example, Jiménez et al. (2014) study the establishment of new bank-firm relationships. I use granular loan-level supervisory data to show that the effects differ in different settings, namely when firms already owe banks some debt. As this debt matures and firms potentially want to roll it over, banks relatively increase credit supply to riskier firms in response to monetary tightening. I argue that this is because banks anticipate the impact of their loan terms on lenders' solvency.

By combining the literature on bank lending reactions to monetary policy with evergreening, I provide valuable insights for policymakers. While the "maturity wall" mentioned in the introduction has so far not led to major financial market turmoil, my results suggest that this may be partly because banks have shielded certain firms with higher default probabilities from increases in policy rates. While this alleviates concerns in the short run, it calls for caution in the long run, as it reduces profits for banks and pushes banks' lending portfolios towards more risky lending at lower risk compensation inherent in the interest rate. This should be the focus for financial stability in the years after monetary tightening. Similarly, central bankers should be aware that monetary policy is transmitted less to riskier firms if they have legacy debt maturing.

As such, this paper is a starting point for potential further research. First, due to data limitations, my results are for the euro area. It would be important to understand whether the effects are similar in other financial systems such as the US, where the role of banks differs and is mainly liquidity provision rather than long-term debt financing. Second, it would be insightful to see how results differ in capital markets with a different structure, such as the more dispersed corporate bond market, where lending is arm's-length and more anonymous (Rajan, 1992). If, as expected, effects are muted in such markets, the push towards a capital markets union and, hence, more market-based financing in the euro area might reduce the importance of this effect in the long run. Third, it would be insightful to understand whether the findings carry over to loan rollovers in the residential mortgage market.

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Appendix 1.A Supplementary Figures and Tables

1.A.1 Supplementary Figures

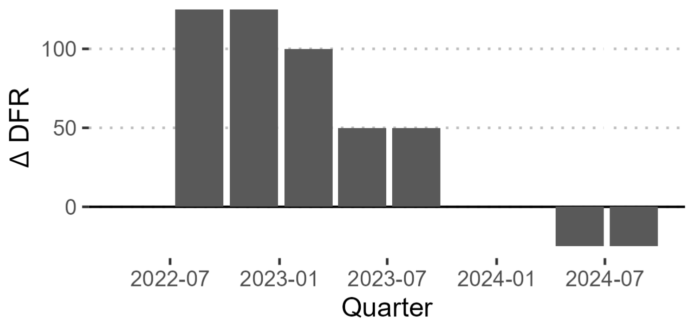

Figure 1.A.1. Changes in the ECB Deposit Facility Rate

Notes: This figure shows the cumulative change in the ECB's Deposit Facility Rate (DFR) for each quarter.

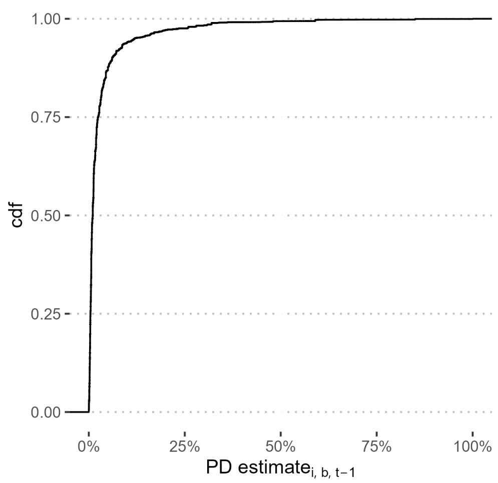

Figure 1.A.2. PD Distribution

Notes: This figure shows the empirical cumulative distribution function for banks' PD estimates. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent.. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans.

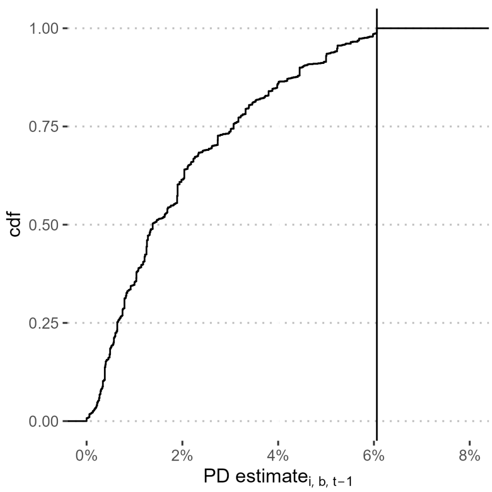

Figure 1.A.3. PD Distribution for the Subsample of Firms for Which at Least One Bank Estimates a PD in the Top Decile

Notes: This figure shows the empirical cumulative distribution function for banks' PD estimates for a subsample of bank-firm relationships. Only estimates below the top decile are included, and only firms for which at least one bank estimates a PD in the top decile. The vertical line depicts the cutoff for the top decile. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent.. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans.

1.A.2 Supplementary Tables

Table 1.A.1. Sample with Single-Bank Firms

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
Monetary policy$_{t-1}$:	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-44.03***	-41.30***	-3.98	-10.48
	(6.26)	(6.61)	(6.32)	(7.81)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$	-1.42**	-2.19**	4.87***	5.92***
	(0.62)	(0.73)	(0.80)	(1.02)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	0.06	0.12***	-0.13**	-0.21***
	(0.04)	(0.03)	(0.06)	(0.04)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Industry-Location-Size-Time FE	Yes	Yes	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes
Adjusted R^2	0.07	0.07	0.07	0.07
R^2	0.20	0.20	0.21	0.21
Observations	9,208,682	9,208,682	8,938,615	8,938,615

Notes: This table shows empirical estimates of equation (1.4.2) where firm-time fixed effects have been replaced with industry-location-size-time fixed effects. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all euro-area firms predominantly financed with fixed-rate loans. The dependent variable in columns (1) and (2) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (3) and (4) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). The monetary policy measure in columns (1) and (3) is *MP shock*$_{t-1}$, which is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. In the remaining columns, the monetary policy measure is *MP shock*$^{\geq 0}_{t-1}$, which is defined in equation (1.3.2). *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. Industry is the 2digit NACE code. The location is the NUTS3 region. Size is a categorical variable for large, medium, small, and micro enterprises according to the Annex to Commission Recommendation 2003/361/EC. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.2. Results with ΔDFR

Dependent variable:	Δ log $\text{Loans}_{i,b,t}$				Δ Interest $\text{rate}_{i,b,t}$			
Single bank firms included:	No		Yes		No		Yes	
Monetary policy_{t-1}:	ΔDFR_{t-1}	$\Delta DFR^{\geq 0}_{t-1}$	ΔDFR_{t-1}	$\Delta DFR^{\geq 0}_{t-1}$	ΔDFR_{t-1}	$\Delta DFR^{\geq 0}_{t-1}$	ΔDFR_{t-1}	$\Delta DFR^{\geq 0}_{t-1}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rollover $\text{need}_{i,b,t}$	-50.965***	-50.286***	-57.547***	-57.071***	-23.872*	-26.055*	-31.880**	-34.553**
	(3.072)	(2.953)	(2.626)	(2.525)	(11.131)	(12.386)	(13.173)	(14.722)
Rollover $\text{need}_{i,b,t}$ × Monetary policy_{t-1}	-0.113***	-0.122***	-0.083***	-0.089***	0.540***	0.525***	0.650***	0.634***
	(0.022)	(0.021)	(0.023)	(0.023)	(0.067)	(0.077)	(0.072)	(0.080)
Rollover $\text{need}_{i,b,t}$ × Monetary policy_{t-1} × PD $\text{estimate}_{i,b,t-1}$	0.004	0.005	0.003**	0.004**	-0.015**	-0.017***	-0.011*	-0.014**
	(0.003)	(0.003)	(0.001)	(0.001)	(0.005)	(0.005)	(0.005)	(0.005)
Firm-Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	No	No	Yes	Yes	No	No
Industry-Location-Size-Time FE	No	No	Yes	Yes	No	No	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	-0.13	-0.13	0.06	0.06	-0.003	-0.003	0.09	0.09
R^2	0.52	0.52	0.21	0.21	0.58	0.58	0.23	0.23
Observations	3,612,609	3,612,609	8,840,669	8,840,669	3,477,024	3,477,024	8,594,022	8,594,022

Notes: This table shows empirical estimates for variants of equation (1.4.4). The sample is a firm-bank-quarter panel from Q2 2022 to Q3 2024 for all multibank euro area firms predominantly financed with fixed-rate loans in columns (1), (2), (5), and (6). In the remaining columns, single-bank firms are also included. The dependent variable in columns (1) to (4) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (5) to (8) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover* $\textit{need}_{i,b,t}$ is defined in eq. (1.3.1). The monetary policy measure in columns (1), (3), (5), and (7) is the cumulative sum of all changes in the ECB deposit facility rate in the previous quarter, measured in bps. In the remaining columns, the monetary policy measure is the cumulative sum of all changes in the ECB deposit facility rate in the previous quarter if this sum is positive and zero otherwise, measured in bps. *PD* $\textit{estimate}_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. Industry is the 2digit NACE code. The location is the NUTS3 region. Size is a categorical variable for large, medium, small, and micro enterprises according to the Annex to Commission Recommendation 2003/361/EC. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.3. Non-linear Impact of the PD: Top Quintile

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
Monetary policy$_{t-1}$:	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-47.90***	-45.88***	6.35	1.03
	(3.93)	(4.20)	(4.91)	(6.06)
Rollover need$_{i,b,t}$ × Monetary policy$_{t-1}$	-1.35***	-1.77***	3.96***	4.74***
	(0.35)	(0.49)	(0.69)	(0.88)
Rollover need$_{i,b,t}$ × Monetary policy$_{t-1}$ × High PD estimate$_{i,b,t}$	0.80**	0.81*	-1.47***	-2.05***
	(0.34)	(0.45)	(0.41)	(0.46)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes
Adjusted R^2	-0.09	-0.09	-0.04	-0.04
R^2	0.51	0.51	0.53	0.53
Observations	4,006,751	4,006,751	3,833,287	3,833,287

Notes: This table shows empirical estimates for variants of equation (1.4.1). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in columns (1) and (2) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (3) and (4) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). The monetary policy measure in columns (1) and (3) is *MP shock*$_{t-1}$, which is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. In the remaining columns, the monetary policy measure is *MP shock*$^{\geq 0}_{t-1}$, which is defined in equation (1.3.2). *High PD estimate*$_{i,b,t-1}$ is a dummy variable defined analogously to equation (1.4.5) that is 1 if the bank's individual lagged estimate of the firm's risk to default within the next year is within the top quintile of the distribution. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.4. Effect on Maturity

Dependent variable:	Δ log Maturity$_{i,b,t}$	
Monetary policy$_{t-1}$:	MP shock$_{t-1}$	MP shock$^{\geq 0}_{t-1}$
	(1)	(2)
Rollover need$_{i,b,t}$	218.25***	222.65***
	(23.40)	(25.16)
Rollover need$_{i,b,t}$ $\times$ Monetary policy$_{t-1}$	-0.38	-2.18
	(1.37)	(1.41)
Rollover need$_{i,b,t}$ $\times$ PD estimate$_{i,b,t-1}$ $\times$ Monetary policy$_{t-1}$	-0.03	-0.02
	(0.02)	(0.03)
Firm-Bank FE	Yes	Yes
Bank-Time FE	Yes	Yes
Firm-Time FE	Yes	Yes
Adjusted R^2	0.12	0.12
R^2	0.60	0.60
Observations	3,828,215	3,828,215

Notes: This table shows empirical estimates of equation (1.4.2). The dependent variable is the quarter-on-quarter log difference in the volume-weighted average maturity of all loans borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all euro-area firms predominantly financed with fixed-rate loans. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). The monetary policy measure in column (1) is *MP shock*$_{t-1}$, which is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. In column (2), the monetary policy measure is *MP shock*$^{\geq 0}_{t-1}$, which is defined in equation (1.3.2). *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.5. Alternative Ways to Measure Rollover Needs

Rollover need$_{i,b,t}$:	Paid off while maturing next quarter		Maturing next quarter	
Dependent variable:	Δ log Loans$_{i,b,t}$	Δ Interest rate$_{i,b,t}$	Δ log Loans$_{i,b,t}$	Δ Interest rate$_{i,b,t}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-41.72***	11.20*	-23.39***	13.60**
	(4.29)	(5.52)	(2.67)	(4.75)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$	-1.16**	3.68***	-0.69**	2.96***
	(0.47)	(0.74)	(0.29)	(0.67)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	0.06*	-0.14**	0.03*	-0.08***
	(0.03)	(0.05)	(0.02)	(0.02)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Adjusted R^2	-0.09	-0.04	-0.10	-0.03
R^2	0.51	0.53	0.50	0.53
Observations	4,006,672	3,833,217	4,006,664	3,833,209

Notes: This table shows empirical estimates of equation (1.4.2). The dependent variable in columns (1) and (3) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in columns (2) and (4) is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. *Rollover need$_{i,b,t}$* in columns (1) and (2) is defined as the share of loan volume maturing and paid off in the current quarter, relative to total loan volume. *Rollover need$_{i,b,t}$* in columns (3) and (4) is defined as the share of loan volume maturing in the current quarter relative to total lagged loan volume. *MP shock$_{t-1}$* is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *PD estimate$_{i,b,t-1}$* is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.6. Controlling for the Firm's Importance for the Bank

Dependent variable:	Δ log Loans$_{i,b,t}$ (1)	Δ Interest rate$_{i,b,t}$ (2)
Rollover need$_{i,b,t}$	-46.24***	3.03
	(4.05)	(5.17)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$	-1.40***	4.02***
	(0.38)	(0.72)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	0.08***	-0.13***
	(0.02)	(0.04)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ $\frac{\text{Loans}_{i,b,t-1}}{\text{Loans}_{b,t-1}}$	-5.78	-9.34
	(15.28)	(12.41)
Firm-Bank FE	Yes	Yes
Bank-Time FE	Yes	Yes
Firm-Time FE	Yes	Yes
Remaining interaction terms	Yes	Yes
Adjusted R^2	-0.10	-0.04
R^2	0.51	0.54
Observations	4,027,180	3,852,172

Notes: This table shows empirical estimates of equation (1.6.4). The dependent variable in the first column is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in the second column is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.7. *PD Estimate* Lagged by Two Quarters

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
PD estimate measure	PD estimate$_{i,b,t-2}$	High PD estimate$_{i,b,t-2}$	PD estimate$_{i,b,t-2}$	High PD estimate$_{i,b,t-2}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-51.02***	-51.01***	2.56	2.81
	(4.90)	(4.47)	(5.43)	(5.56)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$	-1.38**	-1.28**	4.12***	3.92***
	(0.50)	(0.45)	(0.73)	(0.70)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ PD estimate measure$_{i,b,t-2}$	0.08*	1.34**	-0.12***	-1.23**
	(0.04)	(0.53)	(0.03)	(0.50)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Adjusted R^2	-0.10	-0.10	-0.04	-0.04
R^2	0.51	0.51	0.54	0.54
Observations	3,728,505	3,728,505	3,581,000	3,581,000

Notes: This table shows empirical estimates of equation (1.4.2) where the *PD estimate* is lagged by two quarters instead of one. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first two columns is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in the third and fourth column is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *PD estimate*$_{i,b,t-2}$ is the bank's individual estimate of the firm's risk to default within the next year, measured in percentages and lagged by two quarters. *High PD estimate*$_{i,b,t-2}$ is a dummy variable, defined similarly to equation (1.4.5), that is 1 if the bank's individual estimate of the firm's risk to default, within the next year, lagged by two quarters, is within the top decile of the distribution. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.A.8. Raw Monetary Policy Surprises

Dependent variable:	Δ log Loans$_{i,b,t}$		Δ Interest rate$_{i,b,t}$	
Monetary policy$_{t-1}$:	MP shock raw$_{t-1}$	MP shock raw$^{\geq 0}_{t-1}$	MP shock raw$_{t-1}$	MP shock raw$^{\geq 0}_{t-1}$
	(1)	(2)	(3)	(4)
Rollover need$_{i,b,t}$	-46.67***	-45.54***	5.36	2.39
	(4.19)	(4.16)	(6.56)	(6.94)
Rollover need$_{i,b,t}$ × Monetary policy$_{t-1}$	-0.52***	-0.71***	1.24**	1.68***
	(0.14)	(0.14)	(0.49)	(0.38)
Rollover need$_{i,b,t}$ × Monetary policy$_{t-1}$ × PD estimate$_{i,b,t-1}$	0.01	0.03*	-0.03	-0.06***
	(0.02)	(0.02)	(0.02)	(0.02)
Firm-Bank FE	Yes	Yes	Yes	Yes
Bank-Time FE	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes
Remaining interaction terms	Yes	Yes	Yes	Yes
Adjusted R^2	-0.09	-0.09	-0.04	-0.04
R^2	0.51	0.51	0.53	0.53
Observations	4,006,751	4,006,751	3,833,287	3,833,287

Notes: This table shows empirical estimates of equation (1.4.2) with raw monetary policy shocks. The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first two columns is the quarter-on-quarter log difference in the total loan amount borrowed by firm i from bank b in quarter t, multiplied by 100. The dependent variable in the third and fourth column is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm i from bank b in quarter t, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock raw*$_{t-1}$ in columns (1) and (3) is the sum of all high-frequency monetary policy surprises in the previous quarter, provided by Jarociński and Karadi (2020), *not* purged from information surprises and measured in bps. *MP shock raw*$^{\geq 0}_{t-1}$ is defined analogously to equation (1.3.2). *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Appendix 1.B Change in CET1 Ratio After Loss

In the following analysis, I demonstrate that under reasonable assumptions, a loss on a maturing loan negatively impacts the bank's CET1 ratio. Let us define the bank's ex-ante CET1 ratio as $\frac{\text{CET1}}{\text{RWA}}$. Consider a maturing loan with a size L, a risk weight RW, and a loss-given-default LGD. The change in the CET1 ratio resulting from the loan default can be expressed as:

$$\begin{aligned} \Delta\text{CET1 ratio} &= \frac{\text{CET1} - L \cdot LGD}{\text{RWA} - L \cdot RW} - \frac{\text{CET1}}{\text{RWA}} < 0 \\ &\iff \text{RWA} \cdot LGD > \text{CET1} \cdot RW \end{aligned}$$

Assuming further that $\frac{\text{CET1}}{\text{RWA}} \approx 0.15$, we can derive:

$$\begin{aligned} & \text{RWA} \cdot LGD > 0.15 \cdot \text{RWA} \cdot RW \\ \iff & LGD > 0.15 \cdot RW \end{aligned}$$

For instance, even with a high risk weight of 150%, a loss-given-default of $LGD = 0.225$ would suffice for this inequality to hold. Notably, this value is significantly lower than the standard loss-given-default of 45% prescribed by regulations.

Appendix 1.C Loan Data

Cleaning AnaCredit Data. I restrict the sample to loans issued by banks. I define banks as lenders whose institutional sector is either "S122" or "S125". Further, I restrict the sample to non-financial firms, identified via the institutional sector of the borrower starting with "S11" according to ESA 2010 classification and the NACE code of the borrower not being in 64-66. Throughout my analysis, I exclude firms that are in default by requiring the default status of the debtor to be either "Not in default" or missing. I also exclude non-euro area firms because for these firms, my data most likely capture only a minority of the overall bank debt. I define the euro area as the 19 member countries as of 2022, excluding Croatia, which joined only in 2023. Furthermore, I exclude loans for which the loan amount is zero or missing or where the interest is missing. I trim the interest rate at +20% and 0%. I follow Kosekova, Maddaloni, Papoutsi, and Schivardi (2023) and exclude all loans classified as deposits or reverse repurchase agreements or for which the loan type is missing.

There are well-known data quality issues in AnaCredit, which imply that some loans start being reported way later than they should. To deal with these problems, I restrict all analyses to loans that were first reported in the same quarter in which they were settled. For loans settled before 2019, I only require that they were first reported before 2019 since AnaCredit only started at the end of 2018. Thereby, I underestimate the total loan volume but ensure that all loans considered in my analysis are reported in a consistent way. Throughout my analyses, I retrieve only data at the end of quarters and ignore the months in between.

New Loans in AnaCredit. Identifying *new* loans in AnaCredit is challenging but crucial for my analysis. There are data quality issues such that some loans start being reported way later than they should. To deal with these problems, I restrict all analyses to loans that were first reported in the same quarter in which they were settled.[20] Thereby, I underestimate the total loan volume but ensure that all loans considered in my analysis are reported consistently. I then define a new loan as an observation in the same quarter in which the loan was settled.[21]

20. For loans settled before 2019, I only require that they were first reported before 2019 since AnaCredit only started at the end of 2018.

21. An alternative definition would identify new loans as loans that are observed at the inception date. Banks are only required to report loans from the "settlement date" and not from the "inception date". The AnaCredit handbook states: "[In] contrast to the inception date, which is specified in the contract, the settlement date is instrument-specific based on the actual usage of the terms specified under the contract." (European Central Bank, 2019, p. 51) As a result, some loans will only be considered as new loans, according to my definition, some months after the initial contract was signed. However, from the view of the bank and the firm the loan is still new in the sense that it is the first time that money was drawn from this loan.

Construction of a Firm-bank-quarter Panel. Due to the availability of the Jarociński and Karadi (2020) monetary policy shocks, the last quarter of my sample is 2023 Q4. From the loan-level data, I construct a firm-bank-quarter panel by aggregating all loans that a given firm borrows from a given bank in a given quarter. I keep only firms that have loans outstanding with at least one bank in each quarter. This induces some missing values for lags when a firm enters a new bank relationship in a given quarter. I fill missing lags with 0 for loan amount variables.

AnaCredit contains some basic firm information for most firms. I obtain data on the country and region (NUTS3 level), the industry, which I define as the two-digit NACE classification code (following, for example, Degryse, De Jonghe, Jakovljević, Mulier, and Schepens, 2019), and the size of the firm. Size is a categorical variable for large, medium, small, and micro enterprises according to the Annex to Commission Recommendation 2003/361/EC (European Commission, 2003). This size variable is missing for a significant share of observations.

Appendix 1.D Sign-Dependent Effects of Monetary Policy

Formal Test of *Hypothesis 3* . To test *Hypothesis 3* formally, I adjust equation (3) by adding another interaction term. The dummy $\mathbb{1}_{\text{MP shock}_{t-1}>0}$ equals 1 if and only if $\textit{MP shock}_{t-1} > 0$. I adjust the regression equation as follows:

$$\begin{aligned}
\Delta y_{i,b,t} = & \beta_1 \text{Rollover need}_{i,b,t} \\
& + \beta_2 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \\
& + \beta_3 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \beta_4 \text{Rollover need}_{i,b,t} \times \mathbb{1}_{\text{MP shock}_{t-1}>0} \\
& + \beta_5 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \mathbb{1}_{\text{MP shock}_{t-1}>0} \\
& + \beta_6 \text{Rollover need}_{i,b,t} \times \mathbb{1}_{\text{MP shock}_{t-1}>0} \times \text{PD estimate}_{i,b,t-1} \\
& + \beta_7 \text{Rollover need}_{i,b,t} \times \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \times \mathbb{1}_{\text{MP shock}_{t-1}>0} \\
& + \gamma_1 \text{PD estimate}_{i,b,t-1} \\
& + \gamma_2 \text{Rollover need}_{i,b,t} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_3 \text{MP shock}_{t-1} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_4 \mathbb{1}_{\text{MP shock}_{t-1}>0} \times \text{PD estimate}_{i,b,t-1} \\
& + \gamma_5 \text{MP shock}_{t-1} \times \mathbb{1}_{\text{MP shock}_{t-1}>0} \times \text{PD estimate}_{i,b,t-1} \\
& + \eta_{i,b} + \xi_{b,t} + \zeta_{i,t} + \varepsilon_{i,b,t}
\end{aligned} \tag{1.D.1}$$

for firm i borrowing from bank b in quarter t. β_7 provides a formal test of *Hypothesis 3*, as it gives the differential effect of $\textit{PD estimate}_{i,b,t-1}$ on the adjustment of banks' credit supply to firms with rollover needs after contractionary policy shocks compared to expansionary shocks.

Table 1.D.1 shows the results. The significant estimates for β_7 with $\beta_7 > 0$ in column (1) and $\beta_7 < 0$ in column (2), this confirms *Hypothesis 3*.

Table 1.D.1. Sign-Dependent Effects of Monetary Policy: Dummy

Dependent variable:	Δ log Loans$_{i,b,t}$ (1)	Δ Interest rate$_{i,b,t}$ (2)
Rollover need$_{i,b,t}$	-31.28***	-16.37**
	(6.20)	(6.30)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$	4.08**	-2.08
	(1.43)	(1.47)
Rollover need$_{i,b,t}$ $\times$ $\mathbb{1}_{\text{MP shock}_{t-1}>0}$	-18.24***	29.79***
	(5.67)	(8.56)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ PD estimate$_{i,b,t-1}$	-0.12***	0.39***
	(0.04)	(0.10)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ $\mathbb{1}_{\text{MP shock}_{t-1}>0}$	-5.22***	4.72**
	(1.47)	(1.60)
Rollover need$_{i,b,t}$ $\times$ $\mathbb{1}_{\text{MP shock}_{t-1}>0}$ $\times$ PD estimate$_{i,b,t-1}$	0.86**	-1.55***
	(0.33)	(0.42)
Rollover need$_{i,b,t}$ $\times$ MP shock$_{t-1}$ $\times$ $\mathbb{1}_{\text{MP shock}_{t-1}>0}$ $\times$ PD estimate$_{i,b,t-1}$	0.19**	-0.51***
	(0.07)	(0.11)
Firm-Bank FE	Yes	Yes
Bank-Time FE	Yes	Yes
Firm-Time FE	Yes	Yes
Remaining interaction terms	Yes	Yes
Test statistic (p-value) $\beta_3 + \beta_6 + \beta_7 = 0$	0.93 (0.001)	-1.67 (0.000)
Adjusted R^2	-0.10	-0.04
R^2	0.51	0.54
Observations	4,027,180	3,852,172

Notes: This table shows empirical estimates of equation (1.D.1). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in column (1) is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in column(2) is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. $\mathbb{1}_{\text{MP shock}_{t-1}>0}$ equals 1 if and only if MP shock$_{t-1} > 0$. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Table 1.D.2. Sign-Dependent Effects of Monetary Policy: Separate Variables

Dependent variable:	Δ log Loans$_{i,b,t}$ (1)	Δ Interest rate$_{i,b,t}$ (2)
Rollover need$_{i,b,t}$	-43.49***	3.31
	(4.61)	(6.99)
Rollover need$_{i,b,t}$ $\times$ max(MP shock$_{t-1}$, 0)	-2.01***	4.01***
	(0.63)	(0.81)
Rollover need$_{i,b,t}$ $\times$ min(MP shock$_{t-1}$, 0)	0.42	4.00
	(0.98)	(3.19)
Rollover need$_{i,b,t}$ $\times$ PD estimate$_{i,b,t-1}$ $\times$ max(MP shock$_{t-1}$, 0)	0.13**	-0.19***
	(0.05)	(0.05)
Rollover need$_{i,b,t}$ $\times$ PD estimate$_{i,b,t-1}$ $\times$ min(MP shock$_{t-1}$, 0)	-0.01	0.06
	(0.06)	(0.17)
Firm-Bank FE	Yes	Yes
Bank-Time FE	Yes	Yes
Firm-Time FE	Yes	Yes
Remaining interaction terms	Yes	Yes
Adjusted R^2	-0.09	-0.04
R^2	0.51	0.53
Observations	4,006,751	3,833,287

Notes: This table shows empirical estimates of equation (1.4.2) where *MP shock*$_{t-1}$ has been replaced with separate regressors for positive and negatives values, *max(MP shock*$_{t-1}$, 0) and *min(MP shock*$_{t-1}$, 0). The sample is a firm-bank-quarter panel from Q1 2021 to Q4 2023 for all multibank euro area firms predominantly financed with fixed-rate loans. The dependent variable in the first column is the quarter-on-quarter log difference in the total loan amount borrowed by firm *i* from bank *b* in quarter *t*, multiplied by 100. The dependent variable in the second column is the quarter-on-quarter difference in the volume-weighted average interest rate on all loans borrowed by firm *i* from bank *b* in quarter *t*, measured in basis points. *Rollover need*$_{i,b,t}$ is defined in eq. (1.3.1). *MP shock*$_{t-1}$ is the sum of all high-frequency monetary policy surprises in the previous quarter, provided and purged from information surprises by Jarociński and Karadi (2020) and measured in bps. *PD estimate*$_{i,b,t-1}$ is the bank's individual lagged estimate of the firm's risk to default within the next year in percent. The remaining interaction terms are included in the estimation but omitted from the table for readability. Standard errors are clustered at the firm-bank and quarter level.

Appendix 1.E Calculation of Monetary Policy-Induced Financing Gap

Effect Through *Total Loans$_{i,b,t}$*. First, I compute the financing gap arising from the fact that a monetary policy shock leads to less favorable loan terms for firms with rollover need, resulting in a lower amount of maturing loan that is rolled over. This is evident from the negative estimate for β_2 on $\Delta \log Loans_{i,b,t}$. Given that I fix $MP\ shock_{t-1} \in \{0, 10\}$, $Rollover\ need_{i,b,t} = 0.02$, and $Loans_{i,b,t-1} = 100$, the effect is a function of $PD\ estimate_{i,b,t-1}$ and whether or not β_3 is switched off. In particular, the financing gap from a 10 bps monetary policy shock is:

$$\begin{aligned}
\text{Financing gap}^{\text{Loans}}_{i,b,t} =& \text{Loans}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) \\
& - \text{Loans}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) \\
\approx& [\Delta \widehat{\log(\text{Loans})}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) + 1] \cdot 100 \\
& - [\Delta \widehat{\log(\text{Loans})}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) + 1] \cdot 100 \\
=& [\Delta \widehat{\log(\text{Loans})}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) \\
& - \Delta \widehat{\log(\text{Loans})}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3)] \cdot 100
\end{aligned}$$

where a positive value denotes a financing gap. I get values for $\Delta \widehat{\log(\text{Loans})}_{i,b,t}$ as fitted values from the regression estimates in column (2) of Table 1.4.2 by plugging in values for $MP\ shock_{t-1}$, $Rollover\ need_{i,b,t} = 0.02$, and $PD\ estimate_{i,b,t-1}$.[22]

Effect Through *Interest rate$_{i,b,t}$*. Second, I compute the financing gap arising from the fact that firms with rollover need pay higher interest rates after a monetary policy shock. I compute it as follows:

$$\begin{aligned}
\text{Financing gap}^{\text{Interest rate}}_{i,b,t} =& \text{Interest expenses}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) \\
& - \text{Interest expenses}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) \\
=& \text{Interest rate}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) \cdot \text{Loans}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) \\
& - \text{Interest rate}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) \cdot \text{Loans}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3) \\
\approx& \frac{1}{4}[\text{Interest rate}_{i,b,t-1} + \Delta \widehat{\text{Interest rate}}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3)] \cdot 100 \\
-& \frac{1}{4}[\text{Interest rate}_{i,b,t-1} + \Delta \widehat{\text{Interest rate}}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3)] \cdot 100 \\
=& \frac{1}{4}[\Delta \widehat{\text{Interest rate}}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 10, \beta_3) - \\
& \Delta \widehat{\text{Interest rate}}_{i,b,t}(\text{PD estimate}_{i,b,t-1}, \text{MP shock}_{t-1} = 0, \beta_3)] \cdot 100
\end{aligned}$$

where for simplicity I assume

$$\text{Loans}_{i,b,t}(\text{MP shock} = 10) = \text{Loans}_{i,b,t}(\text{MP shock} = 0) = 100$$

A positive value denotes a gap. I divide the annual interest rate by 4 to get the quarterly interest expenses. Again, I get values for $\Delta \widehat{\text{Interest rate}}_{i,b,t}$ as fitted values from column (4) of Table 1.4.2.

22. As $\Delta \log(\text{Loans}_{i,b,t}) \approx \frac{\text{Loans}_{i,b,t}}{\text{Loans}_{i,b,t-1}} - 1$, it holds that $\text{Loans}_{i,b,t} \approx (\Delta \log(\text{Loans}_{i,b,t}) + 1) \cdot \text{Loans}_{i,b,t-1}$.

Total Effect. The total financing gap is the sum of the two, i.e.,

$$\text{Financing gap}_{i,b,t} = \text{Financing gap}_{i,b,t}^{\text{Loans}} + \text{Financing gap}_{i,b,t}^{\text{Interest rate}}$$

As predicted values are for the growth rate of total loans in percentages and for the interest rate in basis points, I divide both fitted values by 100.

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Chapter 2

Army of Mortgagors: Long-Run Evidence on Credit Externalities and the Housing Market

Joint with Moritz Kuhn and Farzad Saidi

2.1 Introduction

Housing and mortgage debt are the most important items on the balance sheets of U.S. households. As such, the house price fluctuations of the 21st century have placed the housing and mortgage markets center stage of the debate on the interplay between the financial system and the real economy. In particular, credit supply has been proposed as an important determinant of house prices and, thus, as a key channel for boom and bust cycles of the macroeconomy (Mian and Sufi, 2009). However, the empirical scrutiny of this relationship is burdened with the challenge of separating the role of credit supply from house price expectations that simultaneously govern credit demand. A causal interpretation of empirical estimates thus requires identifying credit expansions that are independent of variation in market participants' house price expectations (Kuchler, Piazzesi, and Stroebel, 2022).

This paper addresses this challenge by leveraging novel data on the universe of mortgages guaranteed under the Veterans Administration (VA) loan program over

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the last three decades. In April 1991, the VA loan program extended eligibility to the large group of Gulf War veterans concentrated in specific regional housing markets. This quasi-experimental variation in credit supply allows to identify the effect of more readily available credit on local house prices. Importantly, this credit supply shock originates from a segment that is separated from the remaining mortgage market only by veteran status, i.e., independent of economic conditions. This unique feature enables us to disentangle house price growth due to credit supply in one segment of the mortgage market from credit fluctuations in the other. In particular, the expansion of credit affects the VA loan segment, but simultaneously shifts house price expectations for the non-VA population in regions with high concentrations of newly eligible veterans due to higher house prices (see, e.g., Armona, Fuster, and Zafar, 2018). In this manner, we document an amplification of the initial house price increases through changes in expectations that feed back to additional credit supply (and demand).

The granularity and the long time span of our data offer two key advantages for our research design. First, we can study the housing market in the 1990s, which unlike the tumultuous 2000s saw anchored house price expectations. Second, we can identify particularly generous loans guaranteed under the VA loan program that offer conditions unmet on the ordinary mortgage market, such as loan-to-value ratios clearly in excess of one. We document a significant positive relationship between the number of generous VA loans and house price growth at the county level, which lasts up to five years, becomes weaker in counties with greater housing supply elasticity, and holds up to including county by decade fixed effects.

To achieve a causal interpretation of this result, we pursue a Bartik-like identification strategy. We construct an instrument for the provision of generous VA loans by interacting a pre-determined exposure measure at the county level with a common shock that varies only over time. In doing so, we consider veterans living on military bases from which any soldiers were deployed to the Gulf War as being more likely to fulfill the eligibility criteria for VA housing benefits.

Combining the microdata with hand-collected data on U.S. military bases, we distinguish VA loan recipients by their military branch (Air Force, Army, Navy, or Marine Corps) and determine, first, for each county the distance to the closest military base of the respective branch from which soldiers were deployed to the Gulf War. Second, we determine the national take-up rate of generous VA loans per branch, which varies over time as veterans served in the military at different ages but purchase homes roughly around the same age (of 30). To ensure that the exclusion restriction holds, we control for any confounding house price effects associated with a county's proximity to military bases in general, as captured by the non-Gulf War equivalent of our Bartik instruments.

We show that a one-standard-deviation higher share of generous VA loans increases house prices by approximately 6% in the year following the credit supply shock. The effect is further amplified for another five years, after which it starts to reverse. We then show that the larger part of this amplification effect reflects house price reactions to developments in the mortgage market that are due to changes in house price expectations. For this purpose, we use our credit-supply-induced exogenous variation in house price growth to scrutinize its impact on the conventional mortgage, as opposed to the VA loan, market. The segmentation of the two mortgage markets allows us to capture the role of house price expectations and beliefs for mortgage market outcomes that potentially foster further house price increases. Consistent with this view, we find that lenders—including those without exposure to the VA loan market—expand credit supply in housing markets with rising prices. A one-standard-deviation larger house price increase leads to a 2.1 percentage-point higher approval rate and 2% lower average interest rates on new mortgages at the county level.

Using application-level data from the Home Mortgage Disclosure Act (HMDA), we show that this finding is robust to controlling for time-varying unobserved heterogeneity at the lender level, including overall trends in individual institutions' lending behavior that are not specific to county-level house price developments. These granular data also allow us not only to dig deeper into underlying heterogeneous effects but also to control for confounding supply and demand forces. Doing so, we establish a net relative increase in supply, which results from multiple forces on both the supply and the demand side of credit.

When we test for demand forces at the mortgage contract level, we exploit between-borrower variation by including fixed effects at the lender by county by year level, which is the most granular level at which mortgage supply can be confounded with local house price growth. In line with Bailey, Dávila, Kuchler, and Stroebel (2019), who fail to detect any effect of increased household optimism on leverage choices among owner-occupiers, we find that demand drops relatively more for owner-occupiers or, put differently, it increases relatively more for non-owner-occupiers, such as expectations-driven investors. This is reflected at the extensive margin by lower approval rates and at the intensive margin by higher loan amounts (conditional on the approval of an application) for the latter type of borrowers.

To isolate supply forces from such demand-driven effects, we incorporate county by year fixed effects, which subsume any stand-alone effect of house price growth on mortgage outcomes, and use between-lender variation in the same county and year. Lenders for whom real estate makes for a larger portion of their overall loan portfolio expand their supply by more in response to credit-supply-induced house price growth. This results in higher approval rates and larger loan amounts granted, even after additionally controlling for lender by year fixed effects. Using complementary data on interest rates from the Federal Housing Finance Agency's Monthly Interest Rate Survey (MIRS), we confirm that such specialized lenders increase their supply and subsequently offer lower interest rates.

Finally, we show that credit-induced house price growth mitigates asymmetric-information concerns in the supply of credit, as lenders charge lower interest rates for new buildings where asymmetric information about the collateral matters more. Importantly, the incorporation of county by year, and in the previous tests lender by county by year, fixed effects holds constant the average effect of (contemporaneous) house prices on households' collateral constraints, the relaxation of which matters for credit supply (see Cloyne, Huber, Ilzetzki, and Kleven, 2019, for evidence from the United Kingdom).

Besides manifesting pecuniary externalities stemming from the VA loan market, these heterogeneous effects are all consistent with the view that house price growth affects mortgage market outcomes through altering beliefs in the form of house price expectations.

Our paper contributes to the literature on the effects of mortgage supply on the macroeconomy. A defining question in this literature is if and how credit supply and house prices connect financial markets and the real economy. In particular, the Great Financial Crisis (GFC) has sparked research trying to model and quantify this connection. Prominently, Mian and Sufi (2009) argue that securitization in the early 2000s translated to a credit supply shock in the housing market, and that this credit supply shock was a substantial driver of the house price boom leading up to the GFC. Rising house prices have then led to increasing credit demand also by other households, further fueling household indebtedness.

In contrast, Foote, Gerardi, and Willen (2012) and Adelino, Schoar, and Severino (2016) argue in favor of a shift in expectations as the main causal mechanism for higher debt levels and house prices. Expectation-driven asset price booms can arise, for instance, from non-rational expectations (e.g., Glaeser and Nathanson, 2017; DeFusco, Nathanson, and Zwick, 2022). The main argument for such an expectation-driven boom is based on the observation that the credit expansion during the house price boom was broad-based across all income strata of the population. A broad-based increase in household debt lends support to the hypothesis that the credit expansion resulted from, rather than caused, higher house prices.[1]

Although the precise mechanism and the initial trigger of the debt and house price booms during the early 2000s are still debated, there is a consensus that the two forces amplified each other and that the resulting high debt levels exacerbated the economic downturn from the GFC. Against the backdrop of this important macroeconomic discussion, there is very little direct evidence on the transmission mechanism—in particular the role of shifting expectations and whether they precede or follow increases in credit supply—and most of the existing evidence focuses on the turbulent times of the boom-bust period surrounding the GFC. The challenge in providing direct evidence is to disentangle expanding credit supply leading to higher house prices from higher house price expectations leading to more credit demand (Adelino, Schoar, and Severino, 2018; Mian and Sufi, 2018). The segmented

1. Violante (2018) discusses the opposing views on the drivers of the debt increase before the GFC.

credit supply shock from the expansion of eligibility for the VA loan program allows us to tackle this challenge and, thus, fill a crucial gap in the literature.

The first building block of our paper is to show how an initial credit supply shock from outside the financial system affects house prices. As such, it is closely related to the strand of research purporting that exogenous credit supply expansions lead to higher house prices (Favara and Imbs, 2015; Di Maggio and Kermani, 2017; Mian, Sufi, and Verner, 2020; Blickle, 2022). Unlike our setting, these papers have in common that they rely on credit supply shocks that originate from the banking sector, e.g., due to regulation or changes affecting local bank competition.[2] More akin to the nature of our credit supply shock is that in Tracey and Van Horen (2021), who study the "Help to Buy" program in the United Kingdom during the aftermath of the GFC. Likely because of the challenging financial market conditions, the program provided support for potential homeowners aiming to buy houses with low downpayments who otherwise would not have received financing given the then predominant market conditions—typically young, low-income households.

By using the expansion of VA eligibility in the early 1990s, our approach is similar in that it relies on a particular historical episode to study the consequences of credit supply shocks. Importantly, however, we exploit a quasi-experimental expansion of credit that results from past geopolitical decisions of the U.S. government and is, thus, orthogonal to the financial system. Furthermore, the shock affects only a clearly defined segment of the mortgage market. As a result, our VA credit supply shock matches closely the description of a credit supply shock in Mian and Sufi (2018) as "an increased willingness of lenders to provide credit that is independent of the borrowers' income position."

The segmentation of the conventional mortgage market and the VA loan market, in conjunction with the VA eligibility shock, allows us to disentangle the initial effect of credit supply on house prices from the subsequent spillover effects on the remainder of the mortgage market due to adjusted house price expectations. Furthermore, we study the housing market during normal times, which is all the more important in light of evidence that the sensitivity of economic activity to house prices was stable (Guren, McKay, Nakamura, and Steinsson, 2020), unlike most of the existing work that considers time periods around the GFC.[3] Finally, while the Gulf War constitutes as much a singular event as those used in previous studies, we can make use of the fact that the ramifications of the Gulf War for the take-up of generous VA loans materialize even many years later due to variation in the age at which veterans

2. Mian, Sufi, and Trebbi (2010) discuss the political economy of the subprime mortgage expansion before the GFC. The credit expansion of the VA program resulted from geopolitical decisions of the U.S. government.

3. In contrast, Favara and Imbs (2015) study banking deregulation during the 1990s, which they argue allows them to address the potential endogeneity of credit supply to conditions in the housing market.

are drawn into their respective military branch. This puts us in a unique position to consider long-term effects over three decades.[4]

An exception to the approach of looking at particular time periods is Jordà, Schularick, and Taylor (2015) who rely on macroeconomic cross-country panel data and an instrumental-variable approach for shifts in credit supply. They find that across countries and time, house prices and household debt increase after a credit supply shock. Adelino, Schoar, and Severino (2020) also differ from existing work as they use individual-level, rather than regional-level, data to study how changes in financing costs around the conforming loan limit (CLL) affect house prices. They find a positive effect on house prices stemming from lower funding costs, consistent with a positive credit supply shock.

A key advantage of our quasi-natural experiment is that it allows us to separately study and quantify the empirical relevance of the credit supply and expectation-based channels. By identifying a feedback effect of credit-supply-induced house price growth on the conventional mortgage market, we close a gap in the scrutiny of transmission mechanisms of credit supply shocks. Namely, we provide empirical evidence of a key missing link from credit supply to house prices and back to credit supply in line with the expectation-based view, which to date has been only a theoretical conjecture (Violante, 2018).

Mirroring the empirical literature, the theoretical literature also presents different attempts to pin down these two mechanisms. Favilukis, Ludvigson, and Van Nieuwerburgh (2017), Justiniano, Primiceri, and Tambalotti (2019), and Greenwald and Guren (2021) emphasize the quantitative importance of expanding credit supply for the house price boom. Kaplan, Mitman, and Violante (2020) argue that only if there is a sufficiently large group of constrained households, changing credit conditions can drive aggregate house prices. Including both the credit supply and the expectation-based channel in their quantitative model, they conclude that shifts in expectations were the main driver of the house price boom in the early 2000s. They also find a strong effect of rising house price expectations on household debt. By documenting strong pecuniary externalities due to changes in expectations following an otherwise modest credit supply shock, our empirical findings synthesize and reconcile these different theoretical mechanisms underlying the change in aggregate house prices.

Beyond the new economic insights, we also contribute to the literature a novel data source that covers 40 years of U.S. financial history. It is the granularity and extent of these novel data on the universe of VA loans that allow us to expand upon the important findings that already exist on the role of credit in the macroeconomy. The dataset that we introduce relates our work to Fieldhouse, Mertens, and Ravn

4. We will discuss adjusting expectations over the long run, but this is an intricate information problem as non-veteran households have to know the joint distribution of age and eligibility of veterans in their local housing market to form expectations on future credit supply shocks from VA eligibility.

(2018) who use data on changes in GSE mortgage purchases, including those by federal agencies such as the Veterans Administration. They document an increase in mortgage supply and increasing house prices, however solely based on macroeconomic data. To the best of our knowledge, the microdata on VA loan guarantees has not been exploited before for economic research.

2.2 Historical and Institutional Background

The U.S. Department of Veterans Affairs offers a range of services to veterans of the US military. One of the most prominent services is to support veterans in becoming homeowners by providing guarantees for home-purchase and refinancing loans, known as VA loans. The Veterans Administration does not directly grant loans to eligible veterans but, instead, offers insurance for loans of veterans obtained in the private market. Since the program's inception in 1944, more than 22 million loans have been guaranteed. The insured loans offer conditions that are typically not available in the regular mortgage market. Most importantly, the VA does not require any downpayment, making it possible for many borrowers to obtain loans they may not qualify for under other loan-guarantee programs. Eligibility for VA loans is based on veterans' military service, with specific requirements varying by type and duration of service, e.g., having served for at least 90 days on active duty in the Gulf War.[5] As a consequence, large-scale military operations expand the group of eligible veterans. Eligibility increases not automatically, though, but has to be decided by the U.S. Congress.

The Gulf War of 1990-1991 was a significant event in military history. The conflict began when Iraq, under the leadership of Saddam Hussein, invaded Kuwait in August 1990, prompting international condemnation and a military response from the United States and its allies. The role of the U.S. military in the Gulf War was central to the success of the operation, which involved a massive deployment of American troops, equipment, and logistical support to the region. The U.S.-led coalition forces launched two operations:

Operation *Desert Shield* began on August 7, 1990, when the U.S. deployed military forces to the Persian Gulf region in response to Iraq's invasion of Kuwait. The operation was focused on defending Saudi Arabia from potential Iraqi aggression and building up a coalition force to expel Iraqi forces from Kuwait. Operation *Desert Storm* began on January 17, 1991, with an aerial bombardment of Iraqi targets, and continued with a ground assault that liberated Kuwait on February 27, 1991. The success of these operations marked a turning point in the military history of the Middle East and shaped the political landscape of the region for years to come. In total, the U.S. military deployed approximately 700,000 soldiers in both operations,

5. See https://www.va.gov/housing-assistance/home-loans/eligibility/.

making it one of the largest military deployments in history. All four branches of the military—i.e., Air Force, Army, Navy, and Marine Corps—were involved.

Veterans' eligibility for the VA loan guarantee program was historically linked to their having served on active duty during wartime periods. Up until the Gulf War, this would comprise the Mexican Border period (May 9, 1916, to April 5, 1917), World Wars I and II, the Korean conflict, and the Vietnam War era.

We exploit the expansion of the VA loan program subsequent to the Gulf War to quantify the effect of credit supply on house prices and house price expectations in the non-VA segment of the housing market. On April 6, 1991, Public Law 102-25 was enacted, which extended benefits to veterans of the Persian Gulf War, with August 2, 1990, as the beginning date (Section 332). According to Section 341 of that law, a veteran is considered eligible if he served on active duty for at least 90 days, any part of which was during the Persian Gulf War, in addition to 24 months of continuous active duty (or the full period for which the person was ordered to active duty).

The key criterion for eligibility is based on active duty, which is defined as serving in the military full time but does not necessarily imply being deployed. While we cannot observe whether individual soldiers served on active duty, we argue that soldiers living on a military base from which anyone was deployed to the Gulf War are more likely to fulfill the above-mentioned eligibility criteria.[6]

The eligibility expansion subsequent to the Gulf War had a long-lasting effect insofar as it also applies to U.S. veterans involved in the invasion of Afghanistan and Iraq in 2001 and 2003, respectively. In addition, veterans can make use of the VA housing benefit indefinitely, and they may even regain entitlement after paying off the initial loan.

2.3 Data

Our empirical analysis relies on loan-level microdata from two sources that provide high-quality detailed information on loan and borrower characteristics. The first dataset is novel microdata from the VA loan program. The second dataset is the data collected under the Home Mortgage Disclosure Act (HMDA data). We combine a subset of the HMDA data with lender information using the so-called "Avery file." Furthermore, we combine another subset of the HMDA data with interest-rate data from the Federal Housing Finance Agency's Monthly Interest Rate Survey (MIRS). We merge the loan-level microdata with county-level data on house prices, income, unemployment rates, and housing supply elasticities from Saiz (2010). Next, we describe these different data sources in turn.

6. In additional revisions, such as Public Law 102-547 enacted on October 28, 1992, program eligibility was expanded further—so as to include certain reservists—but those expansions do not differentially affect the eligibility of soldiers serving during the Gulf War.

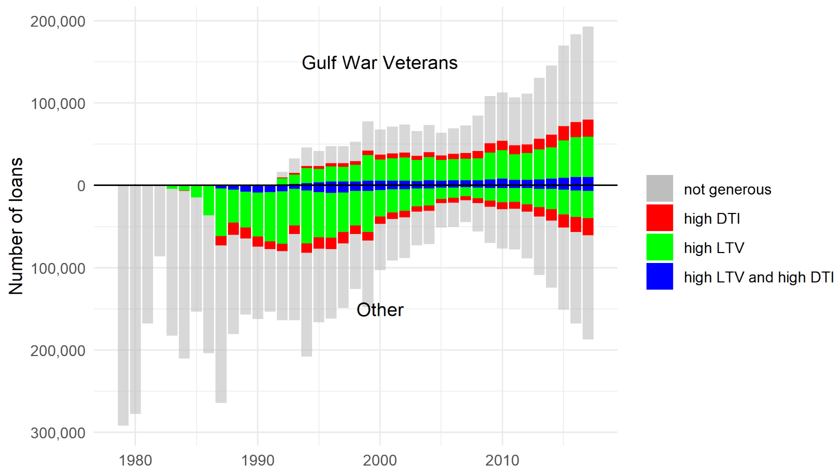
Table 2.3.1. Summary Statistics: VA Loans

	Mean	SD	Min	P25	P75	Max	N
			Generous loans				
Age	31.7	7.1	18.0	26.0	36.0	98.0	1,094,096
Loan amount (in thous.)	191.4	69.3	47.1	137.8	245.4	399.4	1,094,140
Income (in thous.)	69.7	70.1	8.3	49.7	82.5	51,882.4	1,080,244
LTV (in %)	100.9	2.6	79.7	100.7	101.7	102.5	1,094,013
Debt-to-income (in %)	39.7	4.5	25.0	38.1	43.0	43.0	814,675
			Other loans				
Age	34.0	8.1	18.0	28.0	39.0	99.0	1,125,018
Loan amount (in thous.)	204.0	74.7	47.1	144.3	269.6	408.5	1,125,052
Income (in thous.)	80.0	195.2	6.3	53.4	95.3	180,622.9	1,114,065
LTV (in %)	97.3	5.1	79.7	95.1	100.0	102.5	1,124,442
Debt-to-income (in %)	36.0	5.4	25.0	34.1	41.5	41.5	678,514

Notes: The table reports summary statistics for VA loans to Gulf War veterans for home purchases. The upper panel comprises loans classified as generous, whereas the lower panel comprises the remainder. All dollar values are converted to 2017 dollars using the Consumer Price Index Retroactive Series.

2.3.1 Loan-Level Microdata

Figure 2.3.1. Composition of VA Loans over Time



Notes: For each year from 1979 to 2017, this graph plots the number of guaranteed VA loans for home purchases. Positive bars show the number of loans granted to Gulf War veterans. Negative bars show loans to all other veterans. Colored parts of the bars show the number of generous loans broken down by loan characteristics.

The VA loan program data are administered by the Department of Veterans Affairs. We obtain the microdata on the universe of mortgages guaranteed under the

VA loan program for four decades from 1978 to 2017.[7] In total, the data contain 13.3 million records. On average, VA loans correspond to 5-10% of all newly issued mortgages in the U.S. mortgage market. The microdata on these loans provide detailed information on the loan, as is customary also in the HMDA data, but most importantly on the applicant, such as information on the veteran's entitlement status and military branch, which are unavailable in the HMDA data. For our analysis, we focus on the period from 1991 when the first Gulf War entitlement loans are observed in the data up until the end of the sample.[8] There are 3.4 million loans with this entitlement status. Table 2.3.1 reports descriptive statistics of all VA-guaranteed loans granted to Gulf War veterans. Note that some variables such as the loan amount, the loan-to-value (LTV) ratio, or the debt-to-income ratio are only provided in bins in the raw data, and we use the midpoints of these bins to construct data moments.

The upper panel of Table 2.3.1 reports descriptive statistics for loans with particularly "generous" conditions. We will rely on this subset of loans to construct the credit supply shock. The generous VA loans capture the subset of loans insured by the VA program that would typically not be provided in the private market. Specifically, we classify a loan as generous if the debt-to-income (DTI) ratio of the loan is above 43%, which is the maximum permissible ratio given by the Federal Housing Administration (FHA), or if the loan-to-value ratio is above one, implying zero downpayment.[9] These loan conditions are typically not attainable on the ordinary mortgage market as lenders usually require DTI ratios below 43%.[10] Hence, we consider loans with either high LTV, high DTI, or both as generous VA loans.

This is also reflected in the respective summary statistics, as these loans have high LTVs with an average of approximately 101%. For comparison, the average LTV ratio of non-VA mortgages in the first year this variable becomes available in the HMDA data (2018) is 81%, while the average LTV ratio across all VA loans is 97% in the same year. What is more, the average borrower of a generous VA loan is 32 years old and, thus, younger than the average person in the United States. Army veterans at 41% account for the largest share of VA loan borrowers, followed by Navy and Air Force veterans with 23%, while Marine Corps veterans (11%) are least well represented in our sample.[11]

7. Data have been obtained under the Freedom of Information Act request FOIA 22-03431-F.

8. We focus on the entitlement code "Persian Gulf," which also covers the missions in Afghanistan and Iraq in the 2000s.

9. We additionally require that total assets amount to less than 25% of the mean annual income in the same year and county so as to safeguard that borrowers' LTV constraints are binding for conventional loans.

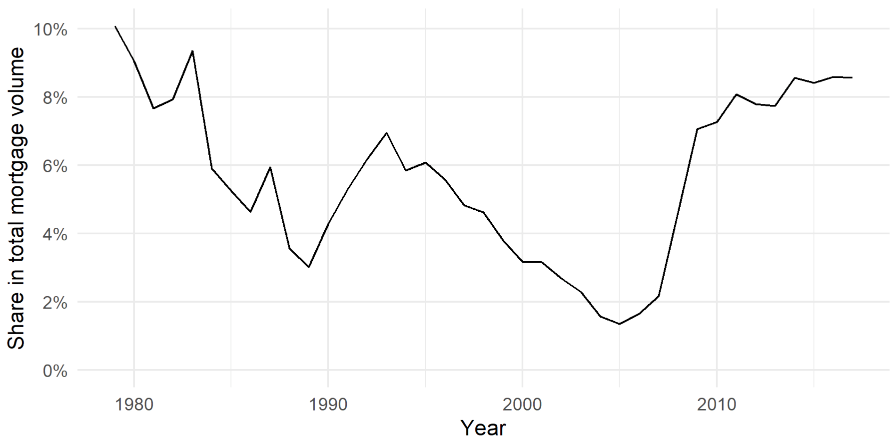
10. A key requirement for income under the VA loan program is the "residual income" of the loan applicant. There exist detailed rules for the determination of residual income, designed to correspond to disposable income of the household after taxes, mortgage payments, utility costs, and other expenditures.

11. Coast guards and other groups account for 2.5% of all VA loans.

The lower panel reports descriptive statistics for all remaining VA loans. They are broadly similar to those in the top panel, owing (at least partly) to the fact that both types of loans are granted to Gulf War veterans. However, against the background of the binned nature of the LTV and DTI ratios, one can still infer that while all VA loans are fairly "generous" compared to ordinary mortgages, this holds in particular for those identified in the top panel. The average LTV ratio is higher for generous loans in the top panel, but the difference is understated due to the bins. Once one zooms in on the middle of the distribution, e.g., the 25th percentile, the difference becomes larger. This holds also for the DTI ratios in the last row of each panel.

Figure 2.3.1 shows the number of VA loans by year of guarantee for different categories. The bars above (below) the horizontal line represent loans granted to Gulf War veterans (all other veterans). The colored bars are generous VA loans broken down by loan characteristics. Notably, generous VA loans were not available prior to the second half of the 1980s. As stated above, many loans are classified as generous because they carry an LTV larger than 100%. Starting in 1992, the number of loans accruing to Gulf War veterans increases quickly up to around 75,000 loans guaranteed each year. While the number of guaranteed VA loans decreases during the housing boom of the 2000s, the number for Gulf War veterans is roughly constant. For our analysis, we focus on home-purchase loans and exclude refinancing loans. Importantly, this implies that we have no subprime loans in our sample.

Figure 2.3.2. Importance of VA Loans in the Total Mortgage Market



Notes: For each year from 1979 to 2017, this figure shows the share of VA loans as a percentage of the total issued mortgage volume. Data from 1979 to 1989, unavailable in the HMDA dataset, are from the U.S. Department of Housing and Urban Development, and data from 1990 to 2017 are from HMDA.

Over the entire period since 1979 (the first year in the VA microdata), the VA loan program covers a substantial share of the U.S. mortgage market. As can be seen in Figure 2.3.2, up to ten percent of all newly issued mortgages are guaranteed by the VA loan program, both before (in the early 1980s) and during our sample period (especially in the 2010s). The VA loan program is, thus, sufficiently large to

Table 2.3.2. Summary Statistics: Conventional Mortgages

	Mean	SD	Min	P25	P75	Max	N
All conventional-loan applications							
Application approved	0.8	0.4	0.0	1.0	1.0	1.0	87,602,221
Loan amount (in thous.)	206.6	258.0	0.0	83.3	265.0	309,000.0	87,602,077
Applicant income (in thous.)	119.9	216.1	1.0	55.0	134.8	542,821.0	87,602,221
Applicant white	0.8	0.4	0.0	1.0	1.0	1	87,602,221
Applicant male	0.7	0.4	0.0	0.0	1.0	1	87,602,221
Home not owner-occupied	0.1	0.3	0.0	0.0	0.0	1.0	87,021,913
Granted loans with interest-rate information							
Interest rate (in %)	6.7	1.2	2.6	5.9	7.5	18.4	4,854,384
Loan amount (in thous.)	225.9	146.2	11.3	126.8	284.4	1,264.2	4,854,384
Maturity (in years)	27.9	5.5	1.0	30.0	30.0	40.0	4,854,384
Loan-to-Price Ratio (in %)	76.2	17.5	2.0	70.0	90.0	100.0	4,854,384
Fixed rate	0.8	0.4	0.0	1.0	1.0	1.0	4,854,384
New building	0.2	0.4	0.0	0.0	0.0	1.0	4,854,376

Notes: The upper panel reports summary statistics for the universe of loan applications in the conventional loan market in the HMDA data at the application level m, as used in Tables 2.5.3 to 2.5.5. The lower panel is limited to the subsample of granted mortgages for which we have data on interest rates from the MIRS dataset, as used in Tables 2.5.6 and 2.5.7. All dollar values are converted to 2017 dollars using the Consumer Price Index Retroactive Series.

make it plausible that changes in the VA loan market can have an effect on prices in the housing market, especially if they give rise to amplification effects through the remaining mortgage market.

To cover the conventional mortgage market, we use the Home Mortgage Disclosure Act (HMDA) dataset. We extract for the time period from 1991 to 2017 data on 88 million loan applications, excluding all subsidized loans (FHA, VA, and FSA/RHS)[12] and, again, any refinancing loans. The upper panel of Table 2.3.2 provides summary statistics for all conventional-loan applications. 80% of all applications are approved on average. Importantly, the average loan amount is close to the average amount of VA loans for Gulf War veterans (cf. Table 2.3.1), but applicants' income is substantially higher for conventional loans given the occupational sorting. Furthermore, the vast majority of loan applications are for owner-occupied housing, which is a characteristic that we use in our empirical analysis to capture relative demand by investment-driven borrowers vs. owner-occupiers. We also include summary statistics on other applicant characteristics, such as their gender, that we use as control variables wherever applicable.

12. See Fieldhouse, Mertens, and Ravn (2018) for a comprehensive overview of the policy changes in these programs over time.

For a subset of these conventional loans, we can add lender balance-sheet characteristics from call reports, which we obtain via Wharton Research Data Services (WRDS). To match the two datasets, we use the HMDA Lender File from the Federal Housing Finance Agency (FHFA), the so-called "Avery file." Using additional information, we determine for each lender the share of real estate loans out of its total loan portfolio and use this share to measure the degree to which the lender specializes in mortgage lending. To overcome endogeneity problems, we use the first observation in a decade for each lender. We find that the share of real estate loans varies significantly across lenders. The median loan portfolio consists of 53% real estate loans, close to the average of 54%. The interquartile range is 34.2 percentage points between 36.4% and 70.6%.

Figure 2.3.3. Distribution of House Price Growth

Notes: For each year from 1991 to 2017, this graph plots the distribution of house price growth across the 2361 counties in our sample. We limit the support of the figure to [-15%;15%].

We obtain interest-rate data from a separate dataset, which cannot be merged with the HMDA data, namely the Federal Housing Finance Agency's Monthly Interest Rate Survey (MIRS) for the period from 1992 to 2010. The MIRS survey is a small-scale survey of mortgage lenders in which respondents are asked to report the terms and conditions of all conventional, single-family, fully amortized purchase-money loans closed during the last five working days of a month. Since participation decreased, the data provide comprehensive coverage only before 2010.[13]

The lower panel of Table 2.3.2 reports the summary statistics for the subsample of granted conventional mortgages with interest-rate information. We find that this subsample aligns closely with the universe of loan applications in the upper panel. The average loan amounts are close at 226 and 207 thousand dollars. Furthermore, loans have an average maturity of almost 28 years. We also include summary statistics on other mortgage characteristics, such as their interest-rate type (fixed vs. floating rate), that we use as control variables wherever applicable. Lastly, 20% of the loans with interest-rate information are used for new buildings, which is a characteristic that we use to capture the extent of asymmetric information.

2.3.2 County-Level Data

We combine the loan-level microdata with regional house prices and local economic conditions, and focus on the county as our unit of analysis. We obtain regional data from Federal Reserve Economic Data (FRED). For each county c we compute annual local house price growth in year t from the house price index by Fannie Mae and Freddie Mac as

$$\text{House price growth}_{c,t} = 100 \times \frac{\text{House price}_{c,t} - \text{House price}_{c,t-1}}{\text{House price}_{c,t-1}}.$$

The index is based on appraisal values and sales prices from mortgages bought or guaranteed, and is computed using the repeated-sales methodology (see Bogin, Doerner, and Larson, 2019, for details). It has an annual, rather than monthly, frequency, which in turn allows for wider geographic coverage and a longer time series than other indices can offer.

Figure 2.3.3 shows the distribution of house price growth across counties for each year from 1991 to 2017. The vertical lines at each year's density mark the 25^{th}, 50^{th}, and 75^{th} percentiles of the house price growth distribution. Over the sample period, we typically observe that most counties saw positive house price growth. On average, broad-based negative house price growth occurs only after the Great Financial Crisis. Furthermore, in all years there is significant variation across counties, with a standard deviation in house price growth of 5.1 and an interquartile range of 5.1.

In addition to house price data, we use county-level population data from the Census Bureau and the unemployment rate from the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS). We calculate mean income at the county level as the total personal income received divided by the county population.

13. For aggregation at the county level, we exclude county-year pairs with fewer than ten observations.

Table 2.3.3. Summary Statistics: County-Year Level

	Mean	SD	Min	P25	P75	Max	N
Average county-level loan statistics							
Share Gulf War VA loans (per 100,000)	27.17	62.06	0.00	3.93	26.85	1,457.46	59,710
Share generous Gulf War VA loans (per 100,000)	12.50	34.97	0.00	0.00	11.90	962.39	59,710
Approval rate conventional loans (in %)	72.77	15.20	0.00	64.34	84.21	100.00	59,665
Mean loan amount, conventional loans (in thous.)	147.44	81.03	11.16	98.93	173.03	2,075.65	59,632
Mean interest rate (in %)	6.84	1.00	3.45	6.12	7.61	11.34	40,054
County economic conditions							
House price growth (in %)	2.89	5.08	−44.81	0.25	5.39	56.42	59,710
Change in unemployment (in pp.)	−0.07	1.22	−13.60	−0.70	0.40	13.20	59,689
Income growth (in %)	3.57	3.71	−85.67	1.85	5.37	89.31	58,421
Population growth (in %)	0.71	1.58	−145.97	−0.16	1.35	35.46	59,710
Housing supply elasticity ρ	2.36	1.24	0.60	1.45	3.00	12.15	7,541
Distance to closest Gulf War base							
Army base (in miles)	338.15	272.42	1.54	145.34	442.75	1,437.97	2,354
Navy base (in miles)	436.31	286.10	2.33	190.86	634.75	1,190.53	2,354
Air Force base (in miles)	246.29	156.04	1.54	123.70	345.31	777.84	2,354
Marine Corps base (in miles)	648.39	319.88	3.39	402.53	915.23	1,376.61	2,354

Notes: Table reports summary statistics at the county-year level *ct*, corresponding to the respective descriptions in Tables 2.4.2 to 2.5.2. Loan amounts are converted to 2017 dollars using the Consumer Price Index Retroactive Series.

We complement these data with housing supply elasticities at the MSA level (Saiz, 2010). To assign counties to their corresponding MSAs, we employ a crosswalk provided by the U.S. Department of Labor, and assume the same housing supply elasticity within all counties belonging to the same MSA (as in Favara and Imbs, 2015). The elasticity is available for about one-third of the counties in our sample. For this subset of counties, we have a mean elasticity of 2.36. When we rely on these elasticities in our analysis, we end the sample in 2000, consistent with the validation period in Saiz (2010). The resulting sample selection is not correlated with distance to the next military base (see Appendix Figure 2.A.1). Finally, Table 2.3.3 provides summary statistics for all county-year-level variables used in our analysis.

2.4 Identification Strategy

The main data source for our analysis of the effects of credit supply on the housing market is the novel administrative VA loan microdata. The VA loan program has two key features that we leverage for this purpose. First, it covers a sizable part of the U.S. mortgage market and is, thus, large enough to have an impact on regional housing markets. Second, the VA loan program only affects a clearly defined segment of the mortgage market. Finally, we exploit for our identification that following the Gulf War, thousands of U.S. veterans became eligible for the VA loan program. This expansion of eligibility of the VA loan program is orthogonal to local economic conditions and the banking sector as the Gulf War itself resulted from U.S. geopolitical decisions.

We are interested in estimating the effect of credit supply on house prices. As a measure of credit supply, we focus on the subset of generous VA loans granted after the expansion of eligibility of the VA loan program. To adjust for the size of local housing markets, we scale the number of generous VA loans by the total population in a county as follows:

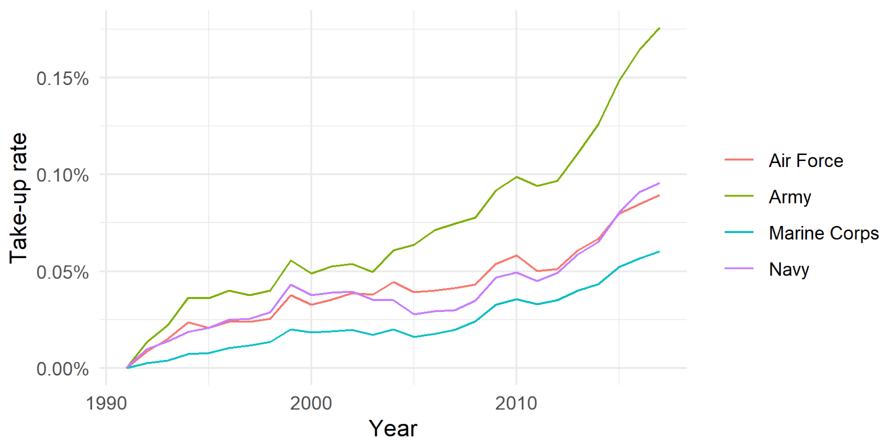
$$\text{VA loans}_{c,t-1} = 100 \times \frac{\text{No. of generous VA loans to Gulf War veterans}_{c,t-1}}{\text{Population}_{c,t-1}}.$$

We then estimate the following county-year-level regression specification:

$$\text{House price growth}_{c,t} = \beta_1 \text{VA loans}_{c,t-1} + \beta_2 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + \varepsilon_{c,t}, \quad (2.4.1)$$

where House price growth$_{c,t}$ and VA loans$_{c,t-1}$ are measured as indicated above, c identifies a county, t indexes calendar years, and $\mathbf{X}_{c,t}$ is a vector of macroeconomic control variables, including change in unemployment, income growth, and population growth. In addition, we control for county by decade fixed effects $\theta_{c,d(t)}$ that capture, for instance, slow-moving demographic factors, and year fixed effects ν_t.

Figure 2.4.1. Take-up Rates of Generous VA Loans by Gulf War Veterans



Notes: For each year from 1991 to 2017, the graph plots the take-up rates of generous VA loans by Gulf War veterans for each military branch at the national level.

A challenge with any measure of credit supply is that the number of issued loans is an equilibrium outcome of the demand for credit and supply thereof. To address this issue, we construct an instrument for VA loans$_{c,t-1}$ at the county-year level as the product of a common shock that varies only over time and a pre-determined, time-invariant exposure measure to this shock that varies across counties. Our instrumental-variables strategy can be interpreted in the spirit of a Bartik-like identification strategy similar to Goldsmith-Pinkham, Sorkin, and Swift (2020).

We compute the common shock as the annual take-up rate of VA loans. To this end, we obtain the number of U.S. veterans from Census data and interpolate the data linearly between census years.[14] When calculating the take-up rate, we follow a leave-one-out approach. We distinguish VA loans by their military branch and construct branch-specific take-up rates:

$$\text{Take-up rate}^{b}_{c,t} \;=\; \frac{\sum\limits_{j\neq c} \text{No. of generous VA loans to Gulf War veterans from branch } b_{j,t}}{\text{Number of U.S. veterans}_t}, \quad (2.4.2)$$

where branch $b \in B = \{\text{Army, Navy, Air Force, Marine Corps}\}$.[15]

As indicated in Table 2.3.1, Gulf War veterans with VA loans exhibit substantial variation in their age. While eligibility for the VA loan program followed a federal decision, individual take-up by eligible Gulf War veterans varies over time as they do not all purchase homes at the same time but, rather, at the same age.

Also, the decision to take out a generous or non-generous loan is likely demand driven and depends on the borrower's financial situation. In Appendix Figure 2.A.2, we show that the share of generous VA loans out of all VA loans varies significantly across lenders. If certain lenders were predominantly issuing generous or non-generous VA loans, we would expect to see higher concentrations at the left and right ends of the distribution.[16]

Figure 2.4.1 plots the annual take-up rates of loans accruing to Gulf War veterans by branch at the national level. Take-up rates are zero before 1992 and then increase constantly over time. In particular, there is a steep increase of take-up rates for loans to Army veterans in the early 2000s. Note that these take-up rates are downward biased due to the fact that we (are forced to) use the total number of U.S. veterans in the denominator (analogously to the county-level definition in (2.4.2)).

To identify the effect of VA loans on house prices, we use the variation in VA loans that is predicted by the pre-determined exposure to the take-up rates. As our exposure measure, we calculate the distance of a county to the closest military base from which soldiers were deployed to the Gulf War. We again construct branch-specific exposure measures, distance^{b}_{c}, where c denotes the county and b the military branch. This reflects the idea that a county that is closer to a Gulf War base is arguably more exposed to the common shock because de-facto deployed soldiers are more likely

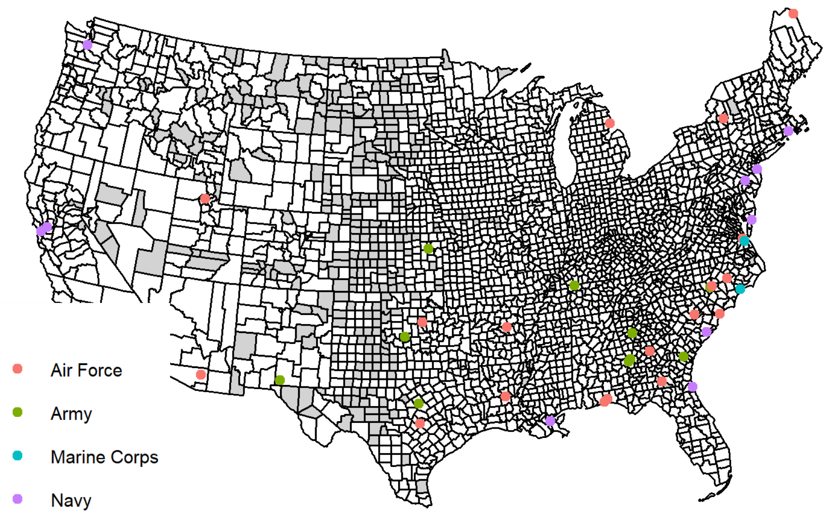
14. Note that the number of veterans is not available by their military branch.

15. The denominator includes all veterans as the number of Gulf War veterans is not available.

16. We can only perform this analysis for loans issued in 2018 because our VA loan data do not include a lender ID and earlier HMDA data lack the variables required to identify generous loans.

to fulfill the eligibility criteria for VA housing benefits. As we show below, these veterans indeed tend to buy homes close to their bases.

Figure 2.4.2. Bases from which Soldiers were Deployed to the Gulf War



Notes: This map shows the location of all military bases in the U.S. from which soldiers were deployed to the Gulf War. The colors represent the different military branches. Grey counties are excluded from our sample either because of missing data or because their population is below 5,000. Table 2.B.1 lists the name, branch, and coordinates of each base.

To compute distance$_c^b$, we hand-collect a list of all military bases based on the Military Bases dataset published by the U.S. Department of Transportation.[17] To identify bases that were active with deployable personnel during the Gulf War, we use reports from Base Realignment and Closure (BRAC) Commission (1991, 1993, 1995, and 2005).[18] We identify 46 military bases from which soldiers were deployed to the Gulf War, alongside their coordinates and military branches.[19] Figure 2.4.2 shows the locations of the Gulf War bases. While the majority of bases are in the East, they exist in all parts of the U.S. Naturally, Marine Corps and Navy bases are concentrated on the coasts. We calculate for each military base its distance from a given county based on the geographical center of the respective county (using the U.S. De-

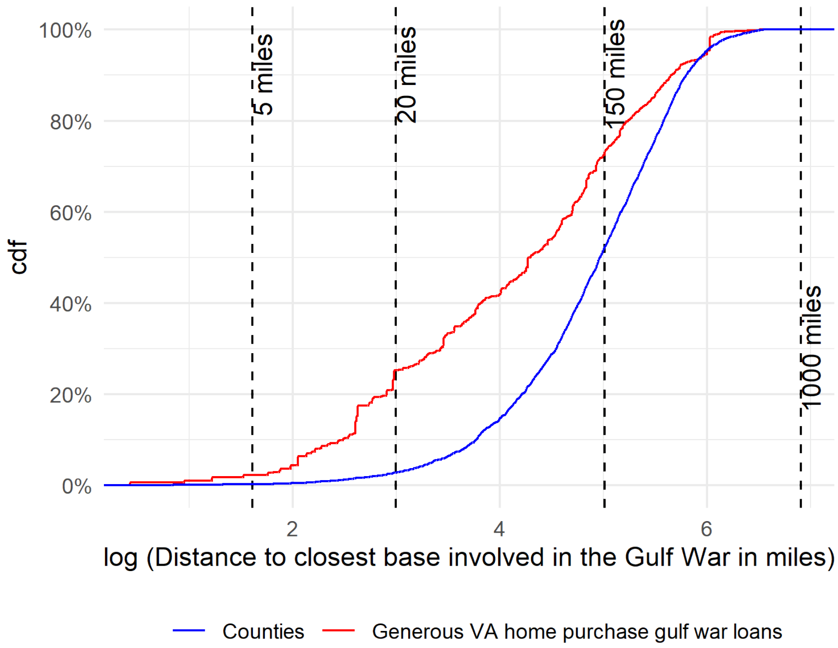
17. See http://public.opendatasoft.com.

18. In cases where the nature of the use of an area is ambiguous, we rely on descriptions from Military.com, newspaper articles, or corresponding Wikipedia entries.

19. See Appendix 2.B for further details. We consider the list to be comprehensive, and could not receive any additional information from the Department of Defense (FOIA 23-F-0965).

partment of Homeland Security's Homeland Infrastructure Foundation-Level Data). We use the natural logarithm of the distance in miles (as in, e.g., Degreyse and Ongena, 2005; Agarwal and Hauswald, 2010; Goetz, Laeven, and Levine, 2013). As we require a valid value for this distance measure for all counties, we exclude Alaska and Hawaii, as well as counties with a population of less than 5,000 inhabitants. Appendix Figure 2.A.3 shows estimated densities for the distance to the closest military base across all counties in our sample. More counties are closer to Air Force and Army bases than to Marine Corps and Navy bases.

Figure 2.4.3. Counties' Distance to Military Bases and Generous VA Loans



Notes: This graph shows empirical cumulative distribution functions of the sum across all years of all generous VA loans to Gulf War veterans (red) and counties (blue) over the log distance to the closest military base from which soldiers were deployed to the Gulf War.

An important prerequisite to safeguard the exogeneity of our credit supply shock is that the location of these bases was pre-determined. Appendix Figure 2.A.4 shows the years of operation for the bases from which troops were deployed. Some bases were established as early as the mid-19th century, and most bases were established during World Wars I and II. The most recent bases began operating in the 1950s. Hence, the location of all bases was chosen at least 30 years before our sample starts.

The 46 military bases constitute a small subset of all military bases in the United States. Anecdotal evidence suggests that deployed units were chosen for military reasons unrelated to local economic conditions.[20]

Combining our measures for the common shock and exposure, we compute four instruments $Z^b_{c,t}$, one for each branch, at the county-year level:

$$Z^b_{c,t} = \log(\text{Distance to closest Gulf War base of branch } b \text{ in miles})^b_c \times \text{Take-up rate}^b_{c,t}. \quad (2.4.3)$$

Table 2.4.1. First-stage IV Results

Dependent variable:		VA loans$_{c,t-1}$	
	(1)	(2)	(3)
$Z^{\text{Army}}_{c,t-1}$	-5.86***	-5.40***	-5.36***
	(1.92)	(1.94)	(2.02)
$Z^{\text{Navy}}_{c,t-1}$	-6.54***	-5.63***	-4.77***
	(1.69)	(1.81)	(1.82)
$Z^{\text{Air Force}}_{c,t-1}$	-4.95*	-4.92	-5.00
	(3.01)	(3.04)	(3.16)
$Z^{\text{Marine Corps}}_{c,t-1}$	4.97	2.81	1.86
	(4.75)	(5.35)	(5.41)
County-Decade FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Local macroeconomic conditions	No	Yes	Yes
Local mortgage market conditions	No	No	Yes
Lagged house price growth	No	No	Yes
F-test (1st stage)	135.8	106.1	97.0
Observations	59,710	58,400	55,432
Adjusted R^2	0.85	0.85	0.85

Notes: The table reports first-stage results of the IV regression (2.5.1) with different control variables. The sample is a county-year panel ct from 1991 to 2017. The instruments are defined as $Z^b_{c,t} = \log(\text{Distance to closest Gulf War base of branch } b \text{ in miles})^b_c \times \text{Take-up rate}^b_{c,t}$ for the four military branches $b \in \{\text{Army, Navy, Air Force, Marine Corps}\}$. The endogenous variable VA loans$_{c,t-1}$ is the relative incidence of generous VA loans. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Local mortgage market conditions include the numbers of conventional loans issued and conventional-loan applications denied per capita, as well as the number of denied applications for FHA loans per capita in county c in the previous year $t-1$. Robust standard errors, clustered at the county level, are in parentheses.

20. For example: "Early on in the process, Saint and Franks had to decide which units in Germany would deploy. The chosen units would not necessarily all come from those currently part of VII Corps. Assigned to the two U.S. Corps in Germany (V and VII) were two armored and two mechanized infantry divisions, two separate brigades, and two armored cavalry regiments, among others. The need for a tank-heavy force, the status of equipment modernization, the state of training, and readiness (specifically the fact that some units were in the process of standing down as part of the downsizing of U.S. forces in Europe) affected Saint's and Franks's decisions." (Source: https://armyhistory.org/jayhawk-goes-to-war-vii-corps-in-operation-desert-storm/)

The identification rests on the exclusion restriction that the distance to military bases must be uncorrelated with the error term, after adding control variables and fixed effects

$$\mathrm{E}[\text{Distance}_c^b \varepsilon_{c,t} | X_{c,t}, \theta_{c,d(t)}, \nu_t] \;=\; 0 \;\forall b \;\in\; B. \tag{2.4.4}$$

Thus, our identification assumption is that the distance of a county to military bases from which soldiers were deployed to the Gulf War affects house prices only through VA loans, which should be valid as deployed units were chosen primarily for military reasons. Furthermore, Bruhn, Greenberg, Gudgeon, Rose, and Shem-Tov (2024) show that deployments to Iraq or Afghanistan at the beginning of the 21$^{\text{st}}$ century had limited effects on soldiers' financial health or education, which could otherwise affect house prices.

In Section 2.C of the Appendix, we consider local government spending (using data from Pierson, Hand, and Thompson, 2015) as another potential confounder with house price growth. We find no evidence of differential local government spending around the Gulf War between counties with and without a Gulf War base.

To further capture any confounding house price effects associated with a county's proximity to military bases in general, we also control for the interaction of the take-up rate with the log distance of county c to the closest military base from which *no* soldiers were deployed to the Gulf War. This helps control for any remaining components in the take-up rate that could be correlated with local economic conditions, in particular local housing demand. Indeed, the only striking difference between these two groups of counties is the prevalence of (generous) VA loans to Gulf War veterans (cf. Appendix Table 2.A.1).

For the relevance of our instrument, it is crucial that veterans tend to buy homes close to their military bases. In Figure 2.4.3, we provide empirical evidence for this assumption. The figure plots the cumulative distribution function of counties and VA loans with respect to the distance to the nearest of all Gulf War bases. The distance of a county to the closest Gulf War base is strongly correlated with the number of VA loans in a county. While only 2.8% of all counties are within 20 miles of a military base, 25.3% of all VA loans to Gulf War veterans with generous conditions were issued in these counties. Hence, the distance to the nearest Gulf War base is a relevant predictor of VA-loan incidence. Appendix Figure 2.A.5 shows that this holds also for each military branch separately. We further evaluate the relevance criterion in Appendix Figure 2.A.6a by scrutinizing the relationship between the instrument for the Army branch and the endogenous variable, VA loans$_{c,t-1}$. There is a clear negative relationship, supporting the relevance of our instrument. This holds also for the remaining three military branches (Appendix Figures 2.A.6b - 2.A.6d).

Finally, we present in Table 2.4.1 the results for the first-stage regression. Depending on the set of control variables, the F-statistic of the joint significance of our instruments varies between 97 and 136. Not all four coefficients on the instruments are negative, however. This is likely driven by the strong positive correlation between the distance measures.

Table 2.4.2. Effect of VA Loans on House Price Growth

Dependent variable:	House price growth$_{c,t}$					
Estimation:	OLS	OLS	OLS	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)
VA loans$_{c,t-1}$	14.9***	10.4***	5.6***	228.3***	207.5***	174.6***
	(2.4)	(1.9)	(1.4)	(49.0)	(49.1)	(44.7)
log(Distance to closest non-Gulf War base)$_c \times$ Take-up rate$_{c,t}^{\text{Gulf War}}$		-101.1**	-133.6***		223.0**	126.5
		(39.7)	(32.1)		(96.3)	(84.1)
County-Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Local macroeconomic conditions	No	Yes	Yes	No	Yes	Yes
Local mortgage market conditions	No	No	Yes	No	No	Yes
Lagged house price growth	No	No	Yes	No	No	Yes
Observations	59,710	58,400	55,432	59,710	58,400	55,432
Adjusted R^2	0.38	0.41	0.49	0.06	0.14	0.29

Notes: The sample is a county-year panel *ct* from 1991 to 2017. Columns 1 to 3 report OLS estimates of (2.4.1) with different sets of control variables and fixed effects. Columns 4 to 6 report IV estimates of (2.5.2), based on the first-stage regression (2.5.1). The dependent variable is the one-year house price growth rate from year t to $t-1$ in %. VA loans$_{c,t-1}$ is the relative incidence of generous VA loans. Local macroeconomic conditions include the change in unemployment rates, income growth, and population growth at the county-year level. Local mortgage market conditions include the numbers of conventional loans issued and conventional-loan applications denied per capita, as well as the number of denied applications for FHA loans per capita in county c in the previous year $t-1$. Robust standard errors, clustered at the county level, are in parentheses.

2.5 Results

In the first part of our empirical analysis, we study the effect of credit on house price growth at the county level. In the second part, we consider the consequences of the credit supply shock for the remaining part of the mortgage market in response to elevated house price expectations.

2.5.1 Credit Supply and House Prices

Table 2.4.2 reports in the first three columns the results from estimating equation (2.4.1) using OLS as a reference point for the discussion. We find throughout a positive and significant effect of credit supply, as measured by the number of generous VA loans, on house price growth. While we always include county by decade and year fixed effects, adding more control variables reduces the coefficient somewhat across columns 1 to 3

The coefficients imply that a one-standard-deviation higher share of generous VA loans corresponds to $(5.6 \times 0.03/5.08 =)$ 3.3% (column 3) to 8.8% (column 1) of a standard deviation higher house price growth (cf. Table 2.3.3). To address the potential endogeneity of these estimates, we use our credit-supply instrument based

Table 2.5.1. Impact of Housing Supply Elasticity on the Effect of VA Loans on House Price Growth

Dependent variable:	House price growth$_{c,t}$		
	(1)	(2)	(3)
VA loans$_{c,t-1}$	113.1***	86.8***	62.9***
	(32.7)	(27.4)	(21.3)
VA loans$_{c,t-1} \times \rho_{msa(c)}$	-33.1***	-29.3***	-18.1***
	(10.2)	(8.9)	(6.7)
County-Decade FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Local macroeconomic conditions	No	Yes	Yes
Local mortgage market conditions	No	No	Yes
Lagged house price growth	No	No	Yes
Observations	7,541	7,294	7,064
Adjusted R^2	0.30	0.40	0.50

Notes: The table reports IV estimates of (2.5.3) with different sets of control variables. The sample is a county-year panel *ct* from 1991 to 2000, consistent with the validation period in Saiz (2010). The dependent variable is the one-year house price growth rate from year t to $t-1$ in %. The endogenous variables are VA loans$_{c,t-1}$, the relative incidence of generous VA loans, and its interaction with $\rho_{msa(c)}$, the housing supply elasticity measure from Saiz (2010) for the MSA corresponding to county c. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Local mortgage market conditions include the numbers of conventional loans issued and conventional-loan applications denied per capita, as well as the number of denied applications for FHA loans per capita in county c in the previous year $t-1$. Robust standard errors, clustered at the county level, are in parentheses.

on generous VA loans, and estimate the following instrumental-variable regression:

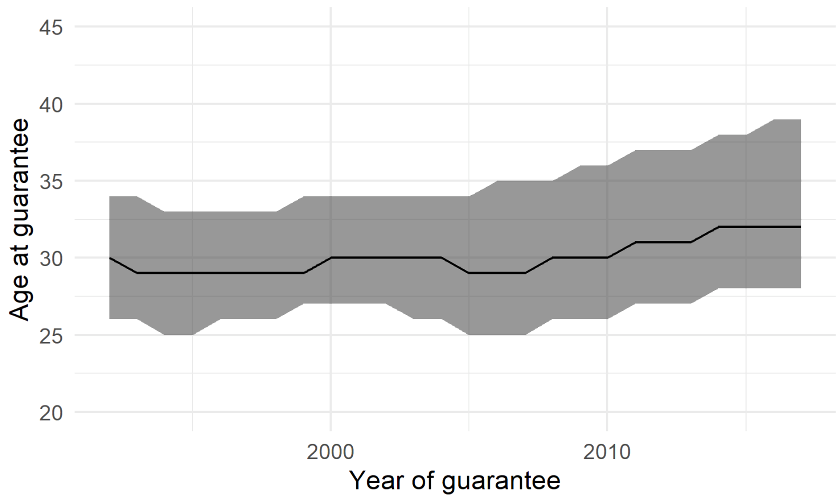
$$\text{First stage:} \quad \text{VA loans}_{c,t-1} = \sum_{b\in B} \gamma_1^b Z^b_{c,t-1} + \gamma_2 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + u_{c,t-1} \tag{2.5.1}$$

$$\text{Second stage:} \quad \text{House price growth}_{c,t} = \beta_1 \widehat{\text{VA loans}}_{c,t-1} + \beta_2 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + \varepsilon_{c,t}, \tag{2.5.2}$$

where $Z^b_{c,t-1}$ is the logged distance to the closest Gulf War base of county c associated with branch b (Army, Navy, Air Force, or Marine Corps) multiplied by the take-up rate in year t, and the remaining variables are defined as in the endogenous regression specification (2.4.1).

The IV results analogous to the OLS specifications in the first three columns are reported in columns 4 to 6 of Table 2.4.2. The IV estimates exceed the OLS estimates by an order of magnitude, and are statistically significant irrespective of the set of control variables and fixed effects. The estimate in column 5 implies that a one-standard-deviation higher share of generous VA loans increases house prices by $(207.5 \times 0.03 =)$ 6.2%, which corresponds to a bit more than one standard deviation in house price growth—a stronger effect than those documented in Di Maggio and Kermani (2017) (3.2%) or Blickle (2022) (3.5%).

Figure 2.5.1. Age of VA Borrowers at Guarantee



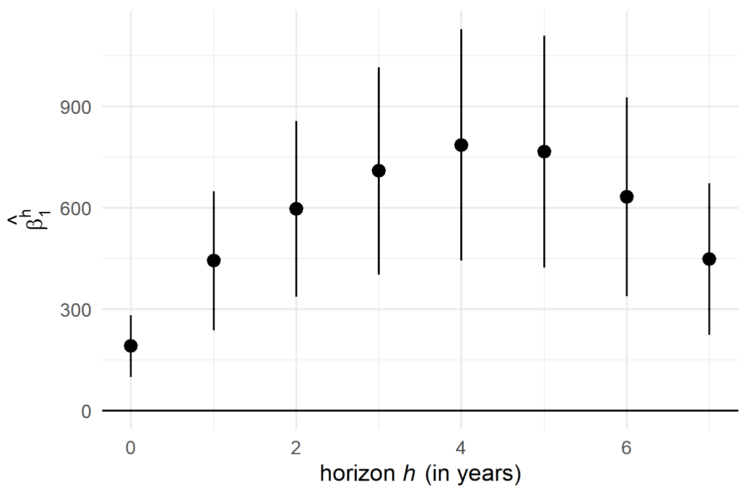
Notes: This figure plots the age at which Gulf War veterans with a generous VA loan receive the guarantee. The solid line shows the median age, and the shaded area depicts the interquartile range.

The coefficient on the non-Gulf War Bartik instrument is negative in the OLS specifications, corresponding to the negative sign of the respective first-stage coefficients in Table 2.4.1. After instrumenting for generous VA loans, our coefficient of interest remains robust, and any negative (positive) effect of the exposure of distant (close) non-Gulf War bases to the take-up rate is explained away as the sign of the respective coefficient flips and eventually becomes insignificant. In Appendix Table 2.A.2, we show that the coefficient of interest is furthermore robust to, first, not controlling for the effect of non-Gulf War bases, second, including year-specific coefficients for the log distance to the closest non-Gulf War base, and, third, interacting this distance with the non-Gulf War instead of the Gulf War take-up rate.

Our results suggest that increasing credit supply to veterans leads to elevated demand for housing, which in turn drives up prices. The increase in prices will depend on the elasticity of the supply of housing, and should be smaller if the supply of housing is more responsive to an increase in demand. To test this, we modify the first-stage specification (2.5.1) and the second-stage specification (2.5.2) by adding an interaction term between the local (MSA-level) housing supply elasticity $\rho_{msa(c)}$ from Saiz (2010) for the MSA corresponding to county c and the instrument (first stage) or VA loans$_{c,t-1}$ (second stage):

$$\begin{aligned}\text{House price growth}_{c,t} \;&=\; \beta_1 \widehat{\text{VAloans}_{c,t-1} \times \rho}_{msa(c)} + \beta_2 \widehat{\text{VA loans}}_{c,t-1} \\ &\quad + \beta_3 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + \varepsilon_{c,t}. \qquad (2.5.3)\end{aligned}$$

Figure 2.5.2. Dynamic Effect of VA Loans on Cumulative House Price Growth



Notes: The dots in this figure are the point estimates for β_1^h in (2.5.4), i.e., local projections of cumulative house price growth on the change in credit supply, for $h \in \{0, 1, 2, ..., 7\}$. The bars represent 95% confidence intervals. Robust standard errors are clustered at the county level.

We expect the effect of an increase in credit supply on house prices to be attenuated when the housing supply elasticity is larger, i.e., $\beta_1 < 0$. Table 2.5.1 presents the results. Across all specifications, the estimated coefficient β_1 is negative and statistically significant. That is, the estimated house price effect is mitigated in counties with greater housing supply elasticity. Given that the average value for $\rho_{msa(c)}$ is 2.36 (cf. second panel of Table 2.3.3), the mitigation effect accounts economically for at least two-thirds of the baseline effect. Hence, we find that in counties where the supply of housing can expand easily the supply of credit leads to less price pressure and, thus, a smaller increase in house prices.

Thus far, we have considered only immediate effects on house prices. It is, however, possible that adjustments in the housing market build up over time and could revert back if, for example, the housing stock adjusts appropriately. That is why in the next step we analyze the dynamic effect of our credit supply shock on house prices.

2.5.2 The Dynamic Response of House Prices

In Figure 2.3.1, we have shown the increasing number of VA loans accruing to veterans of the Gulf War over time. This reflects the idea that not all veterans applied for a mortgage upon becoming eligible. Thus, while the initial credit supply shock expands the availability of credit for many borrowers, not all borrowers demand credit at the same time. An important reason why the one-off expansion in credit supply materializes only over time is that eligible veterans reach the appropriate age for a home purchase in different years. Indeed, most veterans take out a VA loan around the age of 30 (Figure 2.5.1).[21]

To explore the dynamic response of house prices, we estimate local projections of cumulative house price growth on our credit supply shock (Jordà, Schularick, and Taylor, 2020):

$$\begin{aligned} 100 \times \frac{\text{House price}_{c,t+h} - \text{House price}_{c,t-1}}{\text{House price}_{c,t-1}} &= \beta_1^h \widehat{\text{VA loans}}_{c,t-1} \\ &+ \beta_2^h 100 \times \frac{\text{House price}_{c,t-1} - \text{House price}_{c,t-2}}{\text{House price}_{c,t-2}} \\ &+ \beta_3^h \mathbf{X}_{c,t} + \theta^h_{c,d(t)} + \nu^h_t + \varepsilon^h_{c,t}. \end{aligned} \tag{2.5.4}$$

We estimate separate regressions for horizon $h \in \{0, 1, 2, ..., 7\}$, controlling for lagged house price growth. β_1^h captures the cumulative impact of VA loans issued in period $t-1$ on house price growth between $t+h$ and $t-1$.

Figure 2.5.2 shows that generous VA loans have a persistent positive effect on house price growth. The coefficient at $h = 0$ is similar to our IV estimates in columns 4 to 6 of Table 2.4.2. The effect is amplified further thereafter and reverses slowly after five years.

Longer-lived effects beyond $h = 0$ could constitute delayed amplification effects of the initial credit supply shock on house price growth. They could also, however, capture the potential amplification stemming from the reaction of the conventional loan market, to which we turn next.

2.5.3 Mortgage Market Response to House Price Fluctuations and Expectations

Increased eligibility for the VA loan program is a credit supply shock to a segment of the U.S. mortgage market, which we have shown to affect county-level house prices. We now exploit this credit-supply-induced exogenous variation in house price growth to analyze its impact on the conventional mortgage market that does not experience a credit supply shock due to Gulf War veterans' eligibility over time. In doing so, we relate the mortgage market response to changes in house price expectations following the initial shock.

2.5.3.1 Macro-Level Effects

We start out by estimating aggregate effects of (credit-supply-induced) house price growth at the county level. In particular, we wish to analyze the effects of house price growth on (conventional) mortgage applications and loan terms. Since house prices are potentially endogenous to mortgage market decisions, we again employ an IV strategy and use the same set of instruments as in the previous analysis.

21. It is also important to note that not all of the loans in the VA data that have "Persian Gulf" as entitlement were actually issued to veterans who were deployed to the Middle East in 1990-1991.

Table 2.5.2. Effect of House Price Growth on the Conventional Loan Market

Dependent variable:	Approval rate (1)	Δ log(N issued) (2)	Δ log(Loan amount) (3)	Mean interest rate (4)	log(Mean interest rate) (5)	Mean purged interest rate (6)
House price growth$_{c,t}$	0.407***	0.025***	0.029***	-0.027***	-0.004***	-0.024***
	(0.125)	(0.004)	(0.004)	(0.003)	(0.000)	(0.003)
County-Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Local macroeconomic conditions	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,359	58,222	58,222	26,779	26,779	26,779
Adjusted R^2	0.79	0.26	0.22	0.94	0.95	0.91

Notes: The sample is a county-year panel *ct*. The sample period is 1991 to 2017 in columns 1 to 3 and 1992 to 2010 in columns 4 to 6. The table reports IV estimates of (2.5.6). The dependent variable is the approval rate $= 100 \times \frac{\text{No. of issued loans}}{\text{No. of issued loans+No. of denied applications}}$ in column 1, the first difference (between year t and $t-1$) of the logged total number of loans issued in column 2, the first difference (between year t and $t-1$) of the logged total loan amount issued in column 3, the mean interest rate on issued mortgages in levels and logs in columns 4 and 5, and the mean purged interest rate on issued mortgages in column 6. The purged interest rate is the residual from a regression of the interest rate on the logged loan amount, the logged maturity, the LTV, and dummies for the purpose, the lender type, the interest-rate type, and jumbo loans. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. The first-stage regression is defined in (2.5.5). Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Robust standard errors, clustered at the county level, are in parentheses.

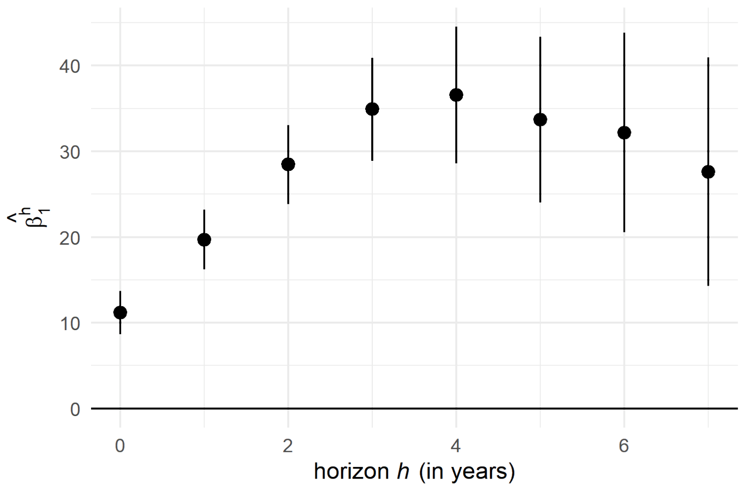
Building on the relevance of our instruments based on credit conditions for U.S. veterans from their deployment to the Gulf War, we directly instrument house price growth, rather than VA loans. The segmentation of the VA and conventional mortgage markets safeguards that the exclusion restriction holds and our instruments affect decisions in the conventional mortgage market only through their effect on house price growth. This justifies the use of the reduced-form equation from the IV strategy in (2.5.1) and (2.5.2) as our new first stage:

$$\begin{aligned} \text{First stage:} \quad \text{House price growth}_{c,t} &= \sum_{b \in B} \gamma_1^b Z_{c,t-1}^b \\ &\quad + \gamma_2 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + u_{c,t} \qquad (2.5.5) \end{aligned}$$

$$\begin{aligned} \text{Second stage:} \quad y_{c,t} &= \beta_1 \widehat{\text{House price growth}}_{c,t} \\ &\quad + \beta_2 \mathbf{X}_{c,t} + \theta_{c,d(t)} + \nu_t + \varepsilon_{c,t}, \qquad (2.5.6) \end{aligned}$$

where $y_{c,t}$ denotes outcome variables from data on the conventional mortgage market (HMDA or MIRS), namely the approval rate, defined as $100 \times \frac{\text{No. of issued loans}}{\text{No. of issued loans+No. of denied applications}}$, the first difference (between year t and $t-1$) of the logged total number of loans issued or of the logged total loan amount issued, and the average interest rate charged on granted mortgages in county c and year t.

Figure 2.5.3. Amplification of Credit-supply-induced House Price Growth



Notes: The dots in this figure are the point estimates for β_1^h in (2.5.7), i.e., local projections of the difference in growth rates between conventional and VA loans on instrumented house price growth, for $h \in \{0, 1, 2, ..., 7\}$. The bars represent 95% confidence intervals. Robust standard errors are clustered at the county level.

Table 2.5.2 presents the results. As house prices increase, so does the approval rate (column 1). This implies that supply increases relative to demand. The number of loans issued and the total volume thereof grow as well (columns 2 and 3). Consistent with a relative increase in supply, the mean interest rate on issued mortgage loans decreases (columns 4 to 6). A one-standard-deviation higher house price growth leads to a 2.1 percentage-point higher approval rate, while it leads to around 14 basis points or 2% lower interest rates.

Based on this empirical strategy, we revisit the dynamic response of house price growth to our initial credit supply shock in Figure 2.5.2. To analyze whether it is driven by an amplification effect within the market for VA loans or due to spillovers to the conventional mortgage market, i.e., an expansion of credit supply in said market that feeds back to higher house prices, we estimate the dynamic effect of (instrumented) house price growth on the growth rate of conventional loans relative to the growth rate of VA loans analogously to (2.5.4):

$$
\begin{aligned}
&100\left(\frac{\text{Conventional loans}_{c,t+h}-\text{Conventional loans}_{c,t-1}}{\text{Conventional loans}_{c,t-1}}-\frac{\text{VA loans}_{c,t+h}-\text{VA loans}_{c,t-1}}{\text{VA loans}_{c,t-1}}\right)\\
=&\beta_1^h 100\times\frac{\widehat{\text{House price}_{c,t}}-\text{House price}_{c,t-1}}{\text{House price}_{c,t-1}}\\
&+\beta_2^h 100\times\left(\frac{\text{Conventional loans}_{c,t-1}-\text{Conventional loans}_{c,t-2}}{\text{Conventional loans}_{c,t-2}}-\frac{\text{VA loans}_{c,t-1}-\text{VA loans}_{c,t-2}}{\text{VA loans}_{c,t-2}}\right)\\
&+\beta_3^h\mathbf{X}_{c,t}+\theta_{c,d(t)}^h+\nu_t^h+\varepsilon_{c,t}^h, \qquad (2.5.7)
\end{aligned}
$$

where Conventional $\text{loans}_{c,t}$ and VA $\text{loans}_{c,t}$ refer to the total loan amount issued in the respective market in county c and year t.

As before, we run separate regressions for horizon $h \in \{0, 1, 2, ..., 7\}$. Furthermore, we winsorize the dependent variable (and its lag on the right-hand side) at the 1st and 99th percentiles.

Figure 2.5.3 shows the results. As instrumented house price growth is measured between t and $t-1$, and the initial credit supply shock is measured in $t-1$, it follows that for $h=0$ we measure growth in the respective credit market in the first year after the shock stemming from greater eligibility for VA loans (as part of our dependent variable). This corresponds to a house price effect in $t+1$ (i.e., $h=1$) in Figure 2.5.2.

A potential concern with this timing could be that the house price effect in $h=0$ reflects not only the initial credit supply shock, but already a reaction by the conventional loan market. To address this despite the unavailability of pre-1991 HMDA data, Section 2.D of the Appendix exploits that the take-up of VA loans by Army veterans during the Iraq War in the 2000s (cf. Figure 2.4.1) may be indicative of their earlier eligibility to show that the conventional loan market does not react to such information, i.e., in anticipation of higher house prices.

Against this background, we can interpret the positive and significant coefficients as indicating that growth in the conventional loan market is greater than in the VA loan market, and that the expansion of credit in the conventional loan market is the stronger force that explains persistent house price growth. This is also reflected in the fact that the local projections exhibit the same pattern. The effect on house price growth reverses after (four to) five years in Figure 2.5.2, which corresponds to the peak at $h=4$ in Figure 2.5.3.

The dynamic pattern is consistent with the idea that the initial credit supply shock stemming from the VA loan market affects house prices on impact, and this effect is amplified by developments in the conventional mortgage market. We next use more granular, application-level data to distill whether these developments stem from higher house price expectations.

2.5.3.2 Transaction-level Results

In Table 2.5.3, we estimate application-level variants of (2.5.6) (cf. (2.5.8) below), and use as dependent variable an indicator variable for whether a mortgage application is approved. As house prices rise, the approval probability increases (column 1), consistent with a relative increase in supply. This also holds when we control for (time-varying) unobserved heterogeneity at the lender level by means of lender (by year) fixed effects (columns 2 and 3).

The effect of house prices on the approval probability is asymmetric: when house prices increase, so does the approval probability. However, when house prices fall, as they do for one-seventh of loan applications, the approval probability also *increases* (cf. negative coefficient on $\widehat{\text{House price growth}}_{c,t}$ in columns 4 to 6). Since lenders are unlikely to respond to falling house prices by increasing the supply of mortgages, this suggests a decline in demand. As house prices fall, households' beliefs change, and the demand for houses due to the speculative motive (Kaplan, Mitman, and Violante, 2020) weakens. When we take this asymmetry into account, the effect of positive house price growth doubles compared to the estimates in the first three columns: when house prices grow by one percentage point, the average approval rate increases by 0.6 percentage points (based on column 6).

The impact of house price growth on the approval probability of loan applications varies strongly over time (columns 7 to 9). The relative increase in supply is largest in the 1990s. In the 2000s, the effect is about half as large (as the coefficient on the intercept effect corresponds roughly to the coefficient on the respective interaction effect). While in the 1990s higher house prices likely led to a decrease in demand through price effects, the housing boom of the 2000s affected households' beliefs and dampened the price effect. Following the Great Financial Crisis, in the 2010s we find no significant effect of house price growth on the approval probability (as the sum of the two respective coefficients is less than zero). This may suggest that borrowers and lenders have become more cautious about house price growth after the experience of the bursting of the housing bubble. Alternatively, supply and demand effects could also offset each other.

To better disentangle the response of supply and demand to house price growth, and provide evidence in line with the view that house price growth affects mortgage market outcomes through altering beliefs, we use the granularity of our data, which allows us to control for confounding supply and demand forces. At the level of mortgage applications m—or at the level of actually granted mortgages when considering their volumes and rates—we estimate the following second-stage regression

Table 2.5.3. Effect of House Price Growth on Approval Rates in the Conventional Loan Market

Dependent variable:	Application approved								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
House price growth$_{c,t}$	0.009***	0.005***	0.003***	-0.016*	-0.011*	-0.005*	0.008***	0.005***	0.003***
	(0.001)	(0.001)	(0.001)	(0.009)	(0.006)	(0.003)	(0.001)	(0.001)	(0.000)
$\mathbb{1}_{\text{House price growth}>0,c,t}$				0.016***	0.000	0.000			
				(0.005)	(0.004)	(0.002)			
House price growth$_{c,t}$ $\times$ $\mathbb{1}_{\text{House price growth}>0,c,t}$				0.028**	0.022**	0.011**			
				(0.013)	(0.009)	(0.004)			
1990s$_t$ $\times$ House price growth$_{c,t}$							0.009***	0.003***	0.003***
							(0.001)	(0.001)	(0.001)
2010s$_t$ $\times$ House price growth$_{c,t}$							-0.022***	-0.009***	-0.005***
							(0.005)	(0.002)	(0.001)
County-Decade FE	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Year FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Lender FE	No	Yes	No	No	Yes	No	No	Yes	No
Lender-Year FE	No	No	Yes	No	No	Yes	No	No	Yes
County FE	No	No	No	No	No	No	Yes	Yes	Yes
Local macroeconomic conditions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Decade-specific distance controls	No	No	No	No	No	No	Yes	Yes	Yes
Observations	86,519,961	86,519,961	86,519,961	86,519,961	86,519,961	86,519,961	86,519,961	86,519,961	86,519,961
Adjusted R^2	0.10	0.26	0.29	0.10	0.25	0.28	0.09	0.26	0.29

Notes: The sample is the universe of all mortgage applications in the conventional loan market at the transaction level m from 1991 to 2017. The table reports IV estimates of (2.5.8). The dependent variable is a dummy for whether the application was granted. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. The dummy $\mathbb{1}_{\text{House price growth}>0,c,t}$ is 1 when house price growth is positive. *1990s$_t$* and *2010s$_t$* are dummies for the respective decades. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Applicant characteristics include a dummy for white applicants, a dummy for male applicants, and the log income of the applicant. Decade-specific distance controls include all interactions of the 1990s and 2010s dummies with the log distance to the closest Gulf War base of each branch. Robust standard errors, clustered at the county level, are in parentheses.

specification, with the first stage being specified analogously to (2.5.5) with twice as many instruments (as the interaction term is also instrumented for by interactions of the instruments and characteristics):

$$\begin{aligned} y_m &= \beta_1 \widehat{\text{House price growth}_{c(m),t(m)} \times \text{Characteristic}_{f(m)}} + \beta_2 \widehat{\text{House price growth}}_{c(m),t(m)} \\ &\quad + \beta_3 \mathbf{X}_{f(m)} + \omega_{f(m)} + \varepsilon_m, \end{aligned} \tag{2.5.8}$$

where $\omega_{f(m)}$ denotes fixed effects at levels that are a function $f(\cdot)$ of the mortgage m itself, always including county (pertaining to the borrower of mortgage m) by decade and year (as determined by the application date of m) fixed effects, and $\text{Characteristic}_{f(m)}$ and $\mathbf{X}_{f(m)}$ are a characteristic and control variables measured at a level that is a function of the mortgage as well.

Higher relative demand should lead to lower application acceptance rates, higher interest rates, but also larger loan volumes. When testing for demand forces, we include interaction effects of House $\widehat{\text{price growth}}_{c(m),t(m)}$ with mortgage- or borrower-specific characteristics, and estimate the following specification:

$$y_m = \beta_1 \widehat{\text{House price growth}_{c(m),t(m)} \times \text{Characteristic}_m} + \beta_2 \mathbf{X}_{f(m)} + \delta_{l(m),c(m),t(m)} + \varepsilon_m, \tag{2.5.9}$$

where Characteristic_m is a characteristic of the borrower of mortgage m, and $\delta_{l(m),c(m),t(m)}$ denotes fixed effects at the lender by county by year level, which is the most granular level at which mortgage supply can be confounded with local house price growth.

As such, β_1 captures relative demand. When testing for supply forces, we control for demand by including county by year fixed effects, which subsume any stand-alone effect of house price growth on mortgage outcomes, while at the same time controlling for time-varying unobserved heterogeneity at the lender level that may govern mortgage outcomes across counties, such as regulatory changes affecting lenders differentially:

$$y_m = \beta_1 \widehat{\text{House price growth}_{c(m),t(m)} \times \text{Characteristic}_{l(m)}} + \beta_2 \mathbf{X}_{f(m)} + \theta_{c(m),t(m)} + \psi_{l(m),t(m)} + \varepsilon_m, \tag{2.5.10}$$

where $\text{Characteristic}_{l(m)}$ is a characteristic of mortgage m that relates, e.g., to lender l, and $\theta_{c(m),t(m)}$ and $\psi_{l(m),t(m)}$ denote county by year and lender by year fixed effects, respectively.

To identify relative supply effects and estimate β_1 in (2.5.10), we use variation at the lender-county-year level. The inclusion of lender by year fixed effects absorbs time-varying unobserved heterogeneity at the lender level that could otherwise bias our estimate. For instance, it precludes that β_1 potentially reflects fluctuations in lenders' net worth due to their exposure to house price developments in a particular county, which may, in turn, affect their lending decisions in other counties.

Thus far, we cannot rule out that our estimates in Tables 2.5.2 and 2.5.3 are driven by the relaxation of collateral constraints—an important credit-supply response due to higher contemporaneous house prices (Cloyne et al., 2019). In the above-mentioned tests, we control for these collateral effects by means of county by year fixed effects, if lenders' response is homogeneous, and at times lender by

Table 2.5.4. Heterogeneous Effect of House Price Growth on Investment-driven Borrowers

Sample	All applications			Issued mortgages		
Dependent variable:	Application approved			log(Loan amount)		
	(1)	(2)	(3)	(4)	(5)	(6)
House price growth$_{c,t}$	0.010***			-0.019***		
	(0.001)			(0.003)		
Home not owner-occupied$_m$ × House price growth$_{c,t}$	-0.007***	-0.002***	-0.001***	0.018***	0.015***	0.015***
	(0.001)	(0.000)	(0.000)	(0.003)	(0.002)	(0.002)
Home not owner-occupied$_m$	0.019***	-0.012***	-0.012***	-0.558***	-0.537***	-0.537***
	(0.005)	(0.002)	(0.002)	(0.017)	(0.016)	(0.016)
County-Decade FE	Yes	No	No	Yes	No	No
Year FE	Yes	No	No	Yes	No	No
County-Year FE	No	Yes	No	No	Yes	No
Lender-Year FE	No	Yes	No	No	Yes	No
Lender-County-Year FE	No	No	Yes	No	No	Yes
Local macroeconomic conditions	Yes	No	No	Yes	No	No
Applicant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	85,945,436	87,021,913	87,021,913	69,115,665	70,036,044	70,036,044
Adjusted R^2	0.10	0.29	0.32	0.45	0.54	0.56

Notes: The sample in columns 1 to 3 is the universe of all mortgage applications in the conventional loan market at the transaction level m from 1991 to 2017. The sample in columns 4 to 7 is the subset of all issued mortgages, i.e., accepted applications. The table reports IV estimates of (2.5.9). The dependent variable in columns 1 to 3 is a dummy for whether the application was granted and the logged loan amount issued in columns 4 to 6. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. Home not owner-occupied$_i$ is a dummy for applicants that will not occupy the home for which they take out the mortgage. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Applicant characteristics include a dummy for white applicants, a dummy for male applicants, and the log income of the applicant. Robust standard errors, clustered at the county level, are in parentheses.

county by year fixed effects, capturing such heterogeneity across lenders. The remaining variation used to estimate our coefficients of interest should stem from altered beliefs, e.g., about expected future collateral values.

Furthermore, if there are lenders that issue both VA loans and conventional mortgages, higher supply of one type of mortgage can crowd out supply of the other (Fieldhouse, 2022) despite the perfect segmentation of the two markets. Besides controlling for this possibility by incorporating lender by year fixed effects, we can more crudely drop lenders that are active in both mortgage markets. In Appendix Table 2.A.3, we show that our results are robust to, first, excluding all loan applications where the lender has issued a VA loan in the year of the application and, second, reducing the sample further by excluding all observations where the lender received an application for a VA loan in any year during our sample period.

In the following, we show heterogeneous supply and demand responses along three dimensions: borrowers' (investment) motives for purchasing a house, lender specialization, and asymmetric information about the underlying collateral value.

Credit demand of investment-driven borrowers. 12% of loan applications are for the purchase of non-owner occupied homes. On average, these borrowers are less constrained because they have higher incomes, both in absolute terms and relative to the loan amount (see Appendix Table 2.A.4 for related summary statistics). As such, their house purchase is more likely to be motivated by investment motives compared to other borrowers, and variation in their beliefs should carry more weight for their mortgage demand than price changes.

In Table 2.5.4, we estimate specifications in the spirit of (2.5.9), and use as the relevant mortgage-level characteristic whether borrowers will not occupy the home (and arguably purchase it as an investment). In this manner, we find that the demand of such borrowers increases at the extensive margin, leading to lower approval rates, in response to rising house prices relative to other borrowers, even after holding constant mortgage supply by adding lender-county-year fixed effects (columns 1 to 3). The demand of borrowers who will not occupy the home increases also at the intensive margin as the loan amount conditional on the approval of an application is larger (columns 4 to 6). Thus, current house prices impact borrowers' demand not only through prices but also through beliefs.

Credit supply by specialized lenders. We can match lenders' balance-sheet characteristics to 16 out of 88 million conventional loan applications. If house price growth encapsulates any valuable information about the state of the housing market in general, lenders that specialize in mortgages should be more prone to updating their beliefs in response to it and adjust their credit supply by more than non-specialized lenders.

In Table 2.5.5, we estimate specifications as in (2.5.10), with $\widehat{\text{House price growth}}_{c,t}$ interacted with lenders' proportion of real estate loans in their total loan portfolio. In line with our hypothesis, the approval probability and the loan amount conditional on issuance increase more for such specialized lenders with a higher share of real estate loans in their loan portfolio. Thus, specialized lenders increase their credit supply more, and should also be less likely to reduce non-housing credit (Martín, Moral-Benito, and Schmitz, 2021), in response to house price growth, along both the extensive and the intensive margin. This holds also when we control for demand through county by year fixed effects.

In our separate dataset on interest rates, we do not have identifiers for lenders and, thus, can neither include lender-identity-based fixed effects nor merge the data with lenders' balance sheets but, instead, have indicators for three different types of lenders: thrifts, mortgage companies, and commercial banks. While thrifts and mortgage companies specialize in mortgages, commercial banks offer a variety of products. This allows us to examine the differential supply response of specialized lenders as reflected by their loan pricing. A greater relative supply effect should be reflected in lower rates. We first show our baseline effect that relative credit supply net-increases in response to higher house price growth in column 1 of Table 2.5.6. In columns 2 to 4, we test for differential credit-supply responses by specialized lenders, and find that they indeed charge lower interest rates in response to higher house price growth, even after controlling for credit demand by including not only

Table 2.5.5. Heterogeneous Effect of House Price Growth on Specialized Lenders

Sample	All applications			Issued mortgages		
Dependent variable:	Application approved			log(Loan amount)		
	(1)	(2)	(3)	(4)	(5)	(6)
House price growth$_{c,t}$	-0.002			-0.015***		
	(0.002)			(0.005)		
$\frac{\text{real estate loans}}{\text{total loans}}_{l(m),t(m)} \times$ House price growth$_{c,t}$	0.011***	0.012***	0.023***	0.017***	0.015**	0.052***
	(0.002)	(0.002)	(0.007)	(0.006)	(0.006)	(0.017)
$\frac{\text{real estate loans}}{\text{total loans}}_{l(m),t(m)}$	-0.037***	-0.047***		-0.028	-0.005	
	(0.008)	(0.009)		(0.024)	(0.024)	
County-Decade FE	Yes	No	No	Yes	No	No
Year FE	Yes	No	No	Yes	No	No
Lender FE	Yes	Yes	No	Yes	Yes	No
County-Year FE	No	Yes	Yes	No	Yes	Yes
Lender-Year FE	No	No	Yes	No	No	Yes
Local macroeconomic conditions	Yes	No	No	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,499,369	15,669,527	15,669,527	13,129,241	13,281,921	13,281,921
Adjusted R^2	0.18	0.19	0.22	0.48	0.49	0.51

Notes: The sample in columns 1 to 3 consists of all mortgage applications in the conventional loan market at the transaction level m from 1991 to 2017 for which we can match lender characteristics. The sample in columns 4 to 7 is the subset of issued mortgages, i.e., accepted applications. The table reports IV estimates of (2.5.10). The dependent variable in columns 1 to 3 is a dummy for whether the application was granted and the logged loan amount issued in columns 4 to 6. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. $\frac{\text{real estate loans}}{\text{total loans}}_{l(m),t(m)}$ is measured at the beginning of each decade. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Applicant characteristics include a dummy for white applicants, a dummy for male applicants, and the log income of the applicant. Robust standard errors, clustered at the county level, are in parentheses.

Table 2.5.6. Heterogeneous Effect of House Price Growth on Interest Rates by Specialized Lenders

Dependent variable:	log(Interest rate)			
	(1)	(2)	(3)	(4)
House price growth$_{c,t}$	-0.005***	-0.001		
	(0.001)	(0.001)		
Specialized lender$_{l(m)}$ × House price growth$_{c,t}$		-0.005***	-0.008***	-0.007***
		(0.001)	(0.001)	(0.001)
County-Decade FE	Yes	Yes	No	No
Lender type-Year FE	Yes	Yes	Yes	Yes
ZIP code FE	Yes	Yes	Yes	No
County-Year FE	No	No	Yes	Yes
ZIP code-Year FE	No	No	No	Yes
Local macroeconomic conditions	Yes	Yes	No	No
Mortgage characteristics	Yes	Yes	Yes	Yes
Observations	4,684,931	4,684,931	4,778,933	4,778,933
Adjusted R^2	0.57	0.57	0.59	0.60

Notes: The sample is a survey of issued mortgages in the conventional loan market at the transaction level m from 1992 to 2010. The table presents IV estimates of a variant of specification (2.5.10), replacing lender with lender-type fixed effects due to the unavailability of lender identities in the MIRS dataset. The dependent variable is the log interest rate. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. Specialized lenders$_{l(m)}$ is a dummy for mortgages that are issued by lenders that specialize in mortgages, i.e., mortgage companies and thrifts, as opposed to mortgages issued by commercial banks. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Mortgage characteristics include the loan-to-price ratio, the log loan amount, the log maturity, a dummy for the interest-rate type (fixed vs. floating), and a dummy for new buildings. Robust standard errors, clustered at the county level, are in parentheses.

Table 2.5.7. Heterogeneous Effect of House Price Growth on Interest Rates and Asymmetric Information

Dependent variable:	log(Interest rate)		
	(1)	(2)	(3)
House price growth$_{c,t}$	-0.005***		
	(0.001)		
New building$_m \times$ House price growth$_{c,t}$	-0.003***	-0.003***	-0.003***
	(0.000)	(0.000)	(0.000)
New building$_m$	0.028***	0.030***	0.032***
	(0.003)	(0.002)	(0.002)
County-Decade FE	Yes	No	No
Lender type-Year FE	Yes	Yes	Yes
ZIP code FE	Yes	Yes	No
County-Year FE	No	Yes	Yes
ZIP code-Year FE	No	No	Yes
Local macroeconomic conditions	Yes	No	No
Mortgage characteristics	Yes	Yes	Yes
Observations	4,684,931	4,778,933	4,778,933
Adjusted R^2	0.57	0.59	0.60

Notes: The sample is a survey of issued mortgages in the conventional loan market at the transaction level m from 1992 to 2010. The table presents IV estimates of a variant of specification (2.5.10), replacing lender with lender-type fixed effects due to the unavailability of lender identities in the MIRS dataset. The dependent variable is the one-year house price growth rate from year t to $t-1$ in %. New building$_m$ is a dummy for new as opposed to existing buildings. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Mortgage characteristics include the loan-to-price ratio, the log loan amount, the log maturity, and a dummy for the interest-rate type (fixed vs. floating). Robust standard errors, clustered at the county level, are in parentheses.

county by year (column 3) but also more granular zip code by year fixed effects (column 4).[22] In both columns, specialized lenders charge almost 1% lower interest rates for each percentage point in house price growth.

Credit supply and asymmetric information. Finally, future house prices should matter more for mortgage supply decisions with higher asymmetric information about the collateral value. To test this, we exploit that in our interest-rate data, 18% of the mortgages are for the purchase of new buildings as opposed to existing buildings, and labeled as such.[23] When house prices are higher, the marginal borrower's loan-to-price ratio may exceed lenders' thresholds and she may, thus, be unable to obtain a mortgage. However, as house prices rise and lenders extrapolate from this into the future, the *expected* future collateral value rises, which can result in lower loan-to-value (LTV) ratios since the value used to calculate regulatory LTV ratios can deviate from the market value of the house at the time of purchase.[24] Such an increase in expected collateral values can, and—as we find on average—does, counteract a reduction in credit supply. Since this effect is stronger for mortgages where asymmetric information about the collateral value (Stroebel, 2016) is more severe, i.e., new buildings, we expect credit supply to increase by more for new, rather than existing, buildings in response to higher house price growth.

The evidence in Table 2.5.7 lends support to this view. First, we find—in line with Stroebel (2016)—that mortgages used to finance the purchase of new buildings carry a higher interest rate. Second, interest rates decrease more for new buildings as house prices rise. Thus, supply increases relative to demand as house prices rise, and more so for mortgages sought for the purchase of new buildings. This holds also when we control for credit demand by means of county by year or zip code by year fixed effects (in column 2 and column 3, respectively).

2.6 Conclusion

This paper revisits the long-standing question on how credit conditions affect house prices and the macroeconomy. We leverage novel and unexplored data from the universe of the Veterans Administration (VA) loan program. The data allow us to construct an instrument for a credit supply shock at the regional housing market level that is independent of economic conditions as it results from the geopolitical decisions of the U.S. government. We find that an expansion of credit supply increases house prices, and then exploit the segmentation of the VA and ordinary mortgage market to trace out the effects of this credit-supply-induced house price growth on the remaining mortgage market. Consistent with the idea that house

22. Note that there are some ZIP codes belonging to more than one county.
23. We use this information as a control variable already in Table 2.5.6.
24. For example, banks often use the "long-term sustainable value" to calculate LTV ratios.

price growth affects expectations, much akin to diagnostic expectations (Bordalo, Gennaioli, and Shleifer, 2018), lenders expand their supply of ordinary mortgages more than demand for credit increases. We show that specialized lenders react more strongly to house price growth and expand their credit supply by more. Future house prices also matter more for mortgage supply decisions with higher asymmetric information about the collateral value such as new buildings as opposed to existing buildings, and for borrowers who mainly purchase a house as an investment such as borrowers who will not occupy the house they purchase.

Our long-run evidence rules in roles for both credit and beliefs in shaping house price cycles, and connects the two by showing that house price growth induced by a credit supply shock affects expectations in the housing market that feed back not only to further credit demand and supply but also contribute to the long-lived nature of house price growth. This opens up the possibility that credit supply can interact with more fundamental forces, which Chodorow-Reich, Guren, and McQuade (2024) highlight in their analysis of the 2000s housing cycle, by steering the path of beliefs.

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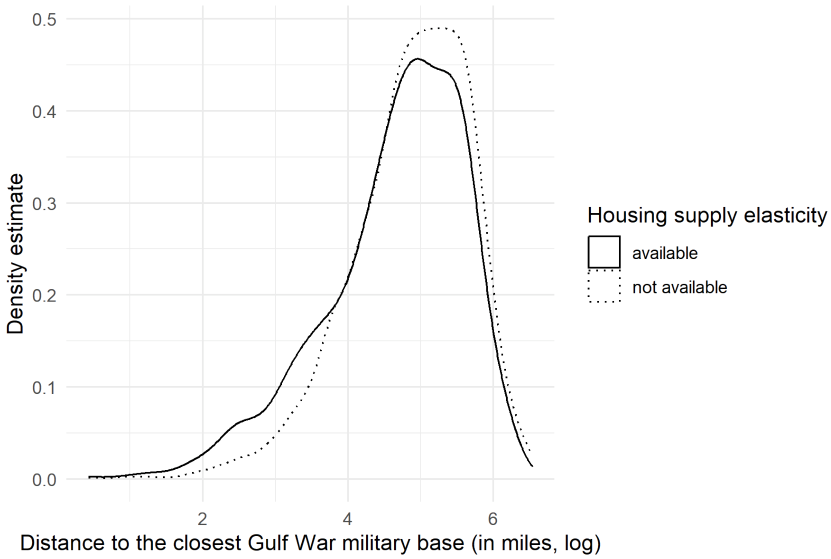
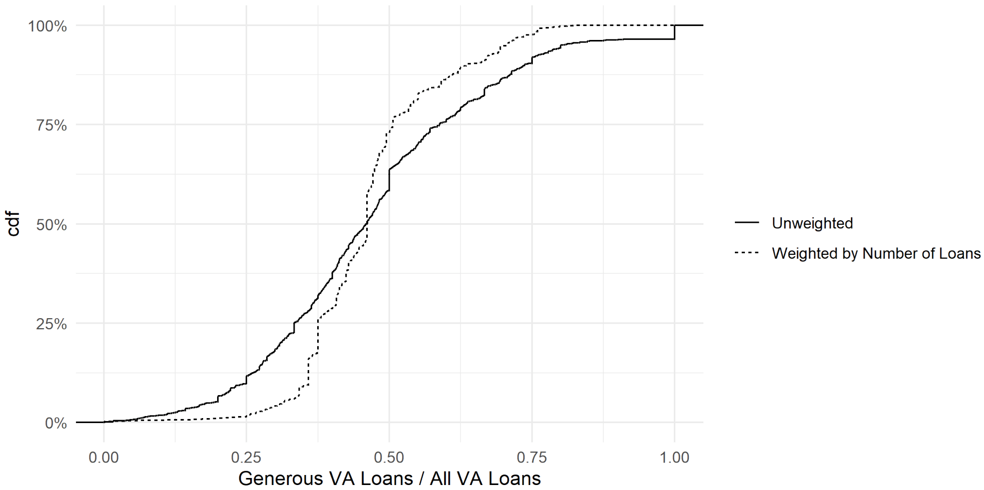
Appendix 2.A Supplementary Figures and Tables

Figure 2.A.1. Distance to Bases and Housing Supply Elasticity



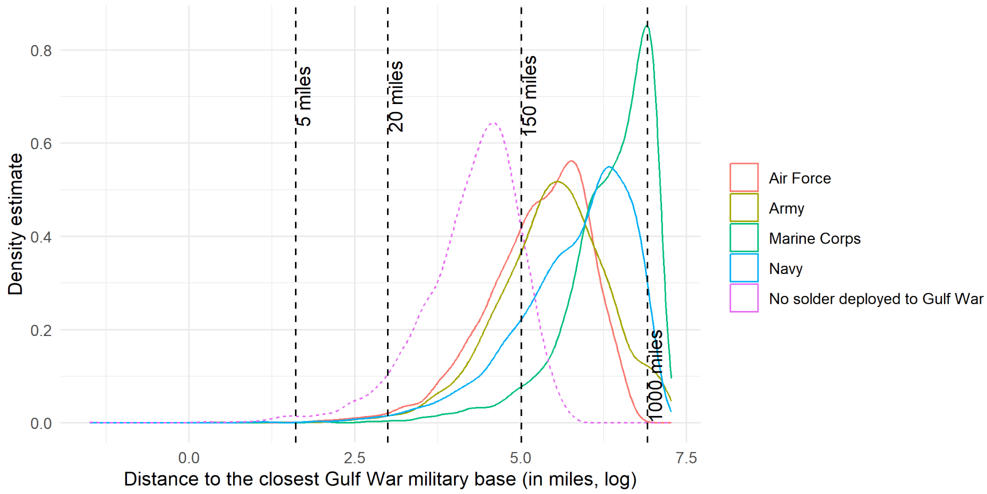
Notes: This figure plots the empirical distribution of the distance to the closest military base across all counties in our sample. The solid line represents counties for which the housing supply elasticity measure $\rho_{msa(c)}$ from Saiz (2010) is available, and the dashed line represents counties for which it is not available.

Figure 2.A.2. Share of Generous VA Loans across Lenders



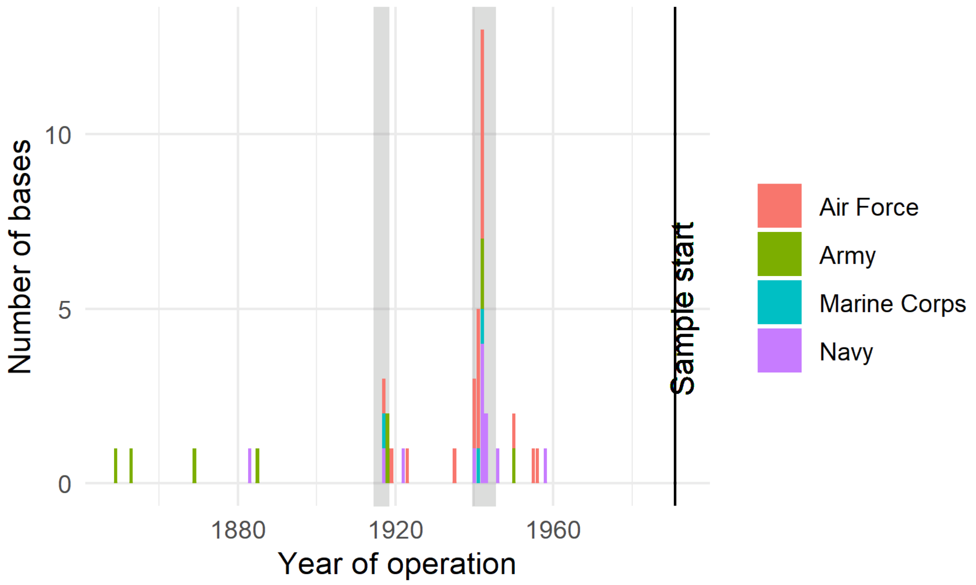
Notes: This graph shows empirical cumulative distribution functions of the share of generous VA loans out of all issued VA loans, across all lenders that reported issued VA loans in HMDA in 2018. Generous loans are defined as loans with an LTV greater than 100% or a DTI greater than 43%.

Figure 2.A.3. Counties' Distance to Military Bases by Branch



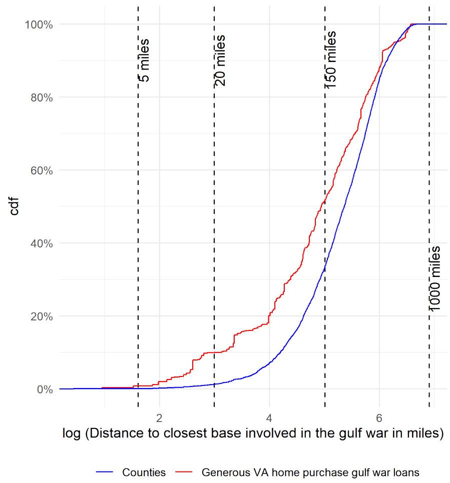
Notes: This figure plots the empirical distribution of distance to the closest military base for each of the four branches across all counties in our sample.

Figure 2.A.4. Years of Operation of Gulf War Military Bases

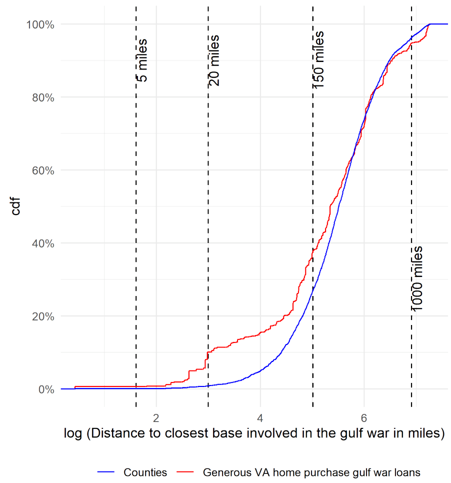


Notes: For each Gulf War military base in our sample, this figure shows the year in which the base began operations. The shaded areas mark World Wars I and II ,and the solid line marks the start of our sample period in 1991.

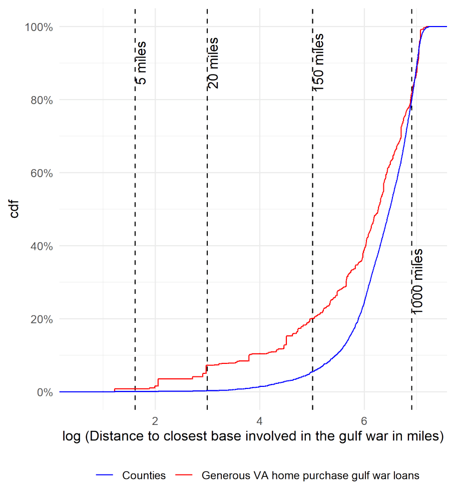
Figure 2.A.5. Counties' Distance to Military Bases and Generous VA Loans by Branch



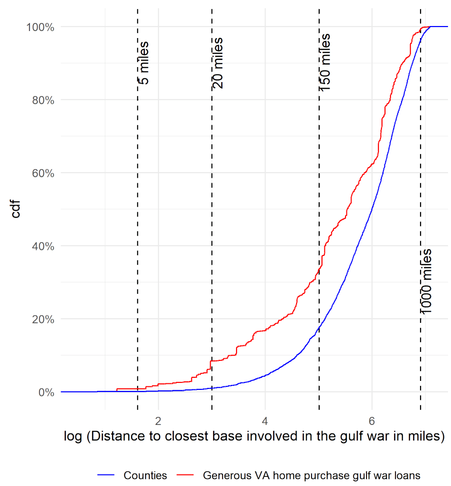
(a) Air Force



(b) Army



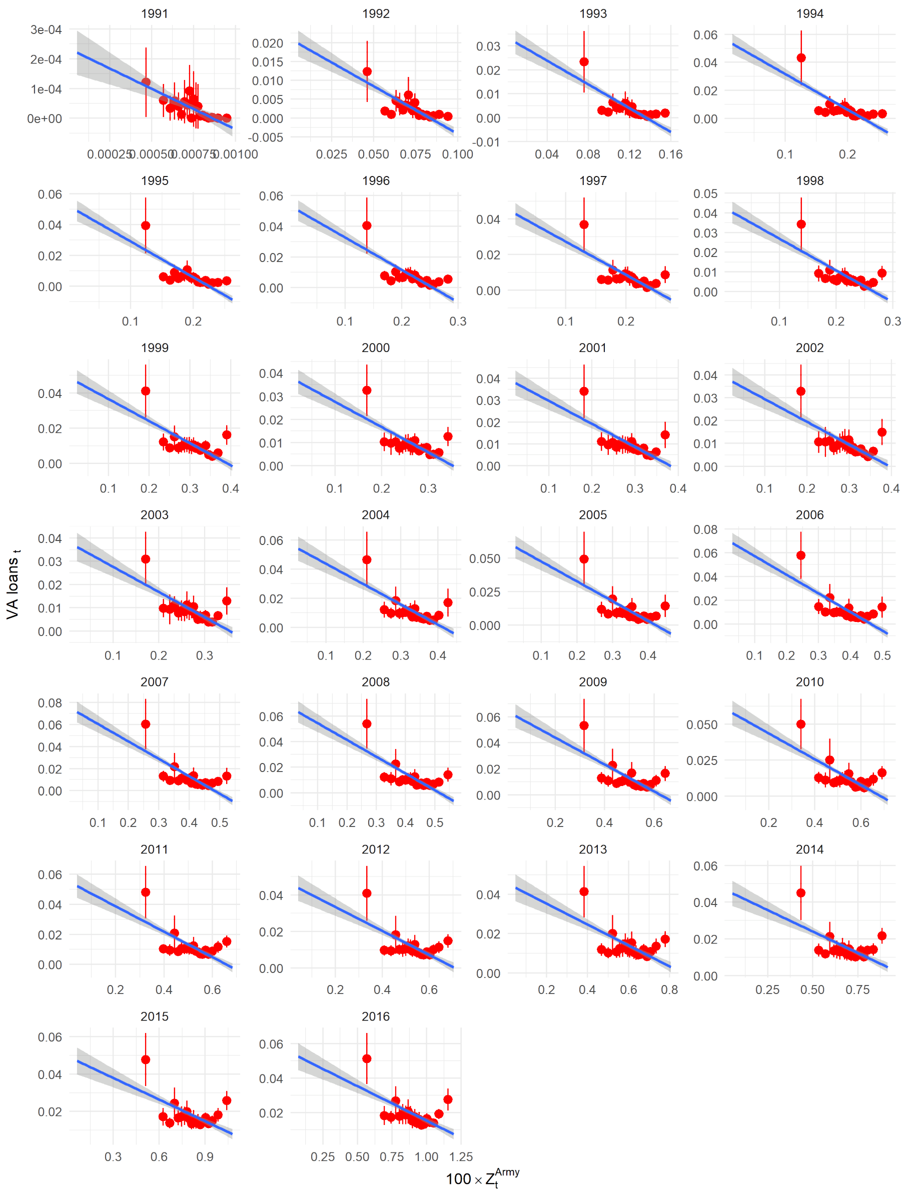
(c) Marine Corps



(d) Navy

Notes: For each of the four military branches, this graph shows empirical cumulative distribution functions of the sum across all years of all generous VA loans to Gulf War veterans (red) and counties (blue) over the log distance to the closest military base from which soldiers were deployed to the Gulf War.

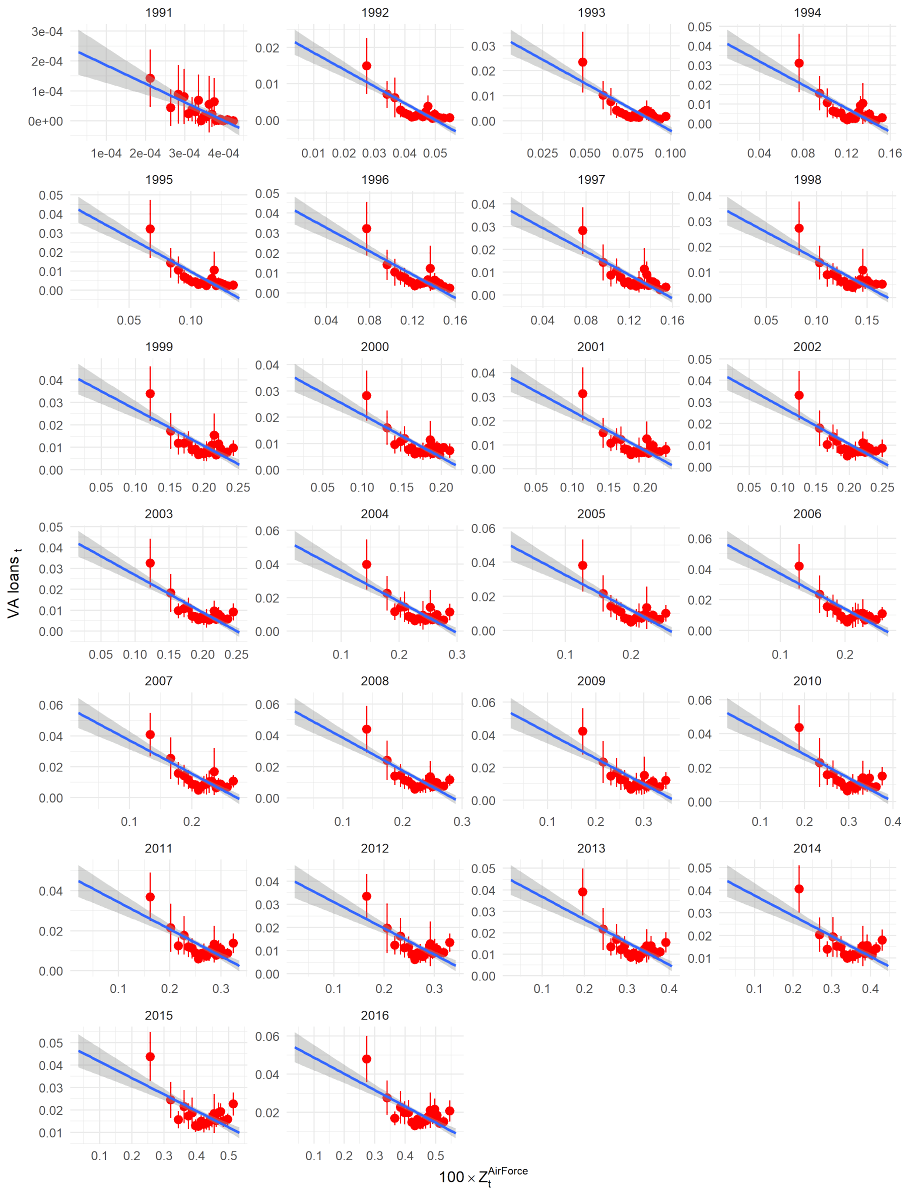
Figure 2.A.6. Instrument for the Different Branches and the Endogenous Variable



(a) Army

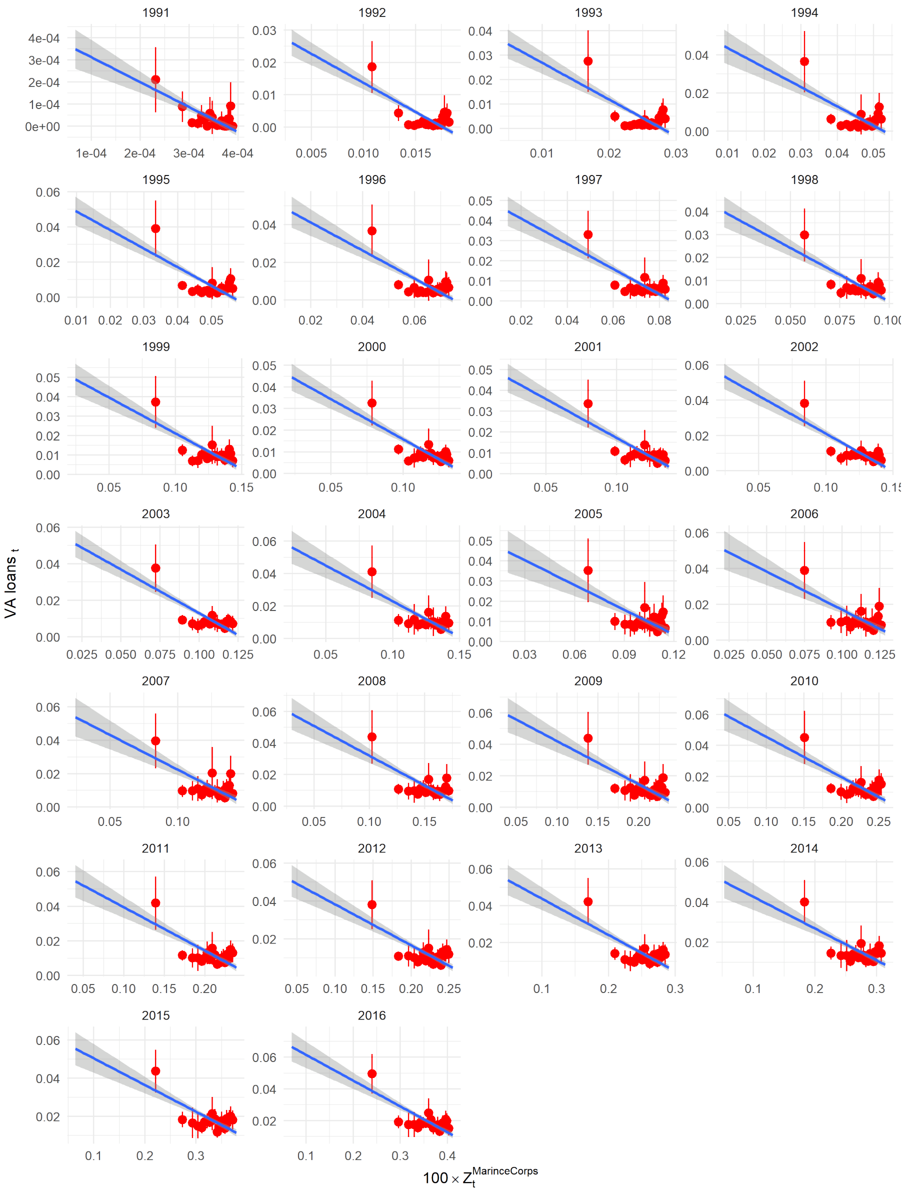
Notes: For each year, this graph shows the relationship between the endogenous variable, VA loans$_{c,t-1}$, which is the relative incidence of generous VA loans, and the instrument for the different branches. The instrument is defined as $Z^b_{c,t} = \log(\text{Distance to closest Gulf War base of branch } b \text{ in miles})^b_c \times \text{Take-up rate}^b_{c,t}$ where $b \in$ {Army, Air Force, Marine Corps, Navy}. The 20 red bins represent local means, and the blue line represents the best linear fit of all observations.

Figure 2.A.6. Instrument for the Different Branches and the Endogenous Variable (ctd.)



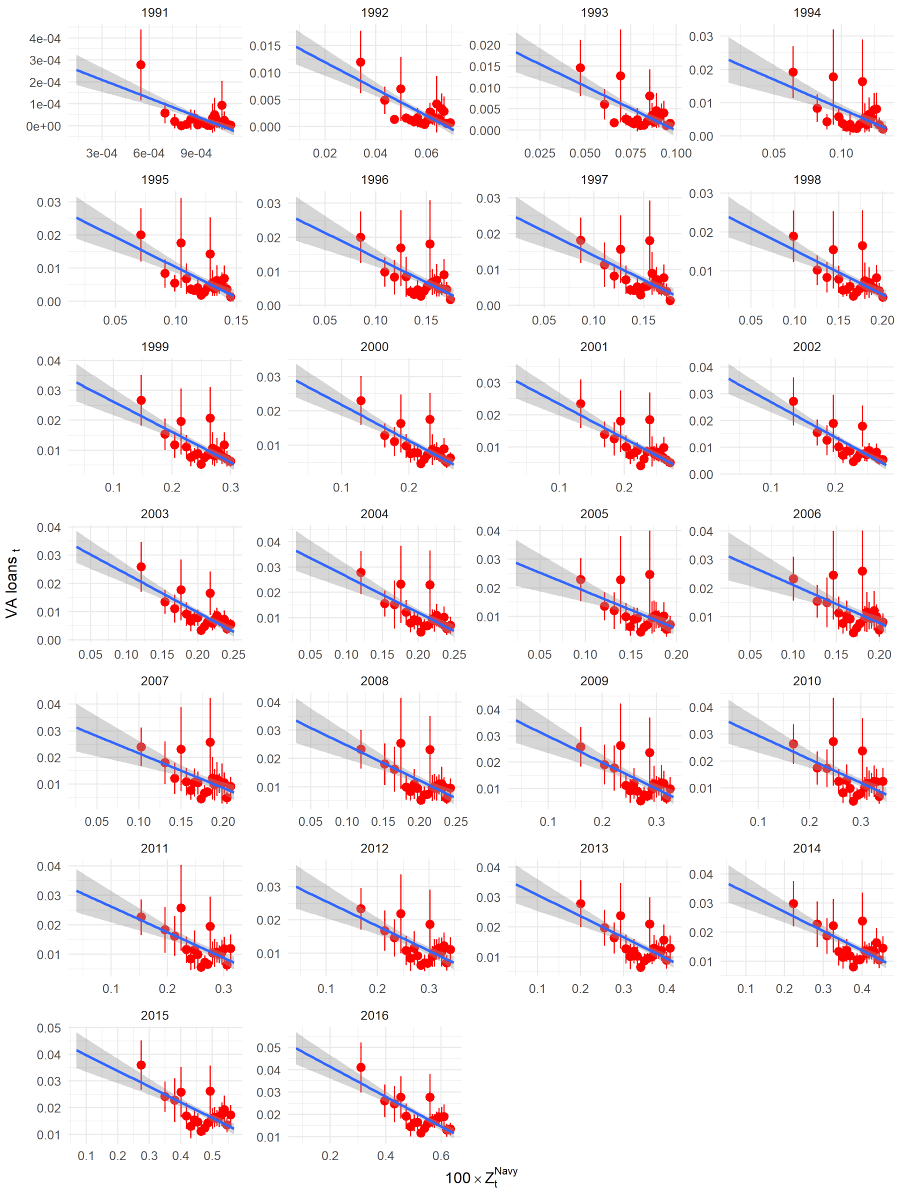
(b) Air Force

Figure 2.A.6. Instrument for the Different Branches and the Endogenous Variable (ctd.)



(c) Marine Corps

Figure 2.A.6. Instrument for the Different Branches and the Endogenous Variable, (ctd.)



(d) Navy

Table 2.A.1. Summary Statistics: County-Year Level by Military Base Status

	Mean	SD	Min	P25	P75	Max	N
				Contains Gulf War base			
Share Gulf War VA loans (per 100,000)	179.88	258.68	0.00	13.23	242.36	1,457.46	1,041
Share generous Gulf War VA loans (per 100,000)	102.43	159.88	0.00	5.67	131.14	962.39	1,041
Approval rate conventional loans (in %)	73.84	13.90	20.69	67.74	83.93	100.00	1,041
Mean loan amount, conventional loans (in thous.)	186.27	98.61	42.32	115.5	225.15	677.32	1,041
Mean interest rate (in %)	6.79	0.97	4.43	6.08	7.58	9.20	729
House price growth (in %)	2.97	5.75	−24.60	−0.10	5.51	37.00	1,041
Change in unemployment (in pp.)	−0.03	1.03	−3.30	−0.70	0.40	6.40	1,038
Income growth (in %)	3.57	3.04	−9.37	2	5.18	23.70	1041
Population growth (in %)	0.86	1.91	−28.05	−0.07	1.83	8.14	1,041
Housing supply elasticity ρ	2.19	1.50	0.67	0.82	3.06	7.15	267
Army base (in miles)	304.96	341.02	1.54	45.91	516.20	1,372.36	39
Navy base (in miles)	222.64	220.51	2.33	20.84	372.12	755.92	39
Air Force base (in miles)	109.36	141.33	1.54	12.85	149.08	692.16	39
Marine Corps base (in miles)	476.54	370.13	3.39	153.16	729.40	1,171.99	39
				Contains non-Gulf War base			
Share Gulf War VA loans (per 100,000)	95.07	115.56	0.00	15.59	131.02	925.82	2,587
Share generous Gulf War VA loans (per 100,000)	48.60	65.19	0.00	6.85	63.29	610.89	2,587
Approval rate conventional loans (in %)	77.84	12.70	23.55	73.64	86.36	100.00	2,586
Mean loan amount, conventional loans (in thous.)	198.94	107.76	35.24	126.9	242.18	1,573.99	2,586
Mean interest rate (in %)	6.76	0.97	4.03	6.07	7.51	8.97	1,807
House price growth (in %)	3.07	6.33	−30.74	0.27	5.64	34.00	2,587
Change in unemployment (in pp.)	−0.05	1.11	−13.50	−0.70	0.40	6.80	2,587
Income growth (in %)	3.45	2.83	−10.61	2.02	5.13	15.58	2,533
Population growth (in %)	0.98	1.36	−12.55	0.22	1.65	9.62	2,587
Housing supply elasticity ρ	2.06	1.08	0.63	1.23	2.71	5.45	639
Army base (in miles)	386.97	297.17	18.19	146.57	580.06	1,326.04	97
Navy base (in miles)	329.43	289.22	10.26	94.89	529.77	1,169.24	97
Air Force base (in miles)	208.96	147.10	8.43	109.82	280.46	715.28	97
Marine Corps base (in miles)	551.79	366.74	15.00	222.37	894.15	1,315.52	97
				Contains no base			
Share Gulf War VA loans (per 100,000)	21.21	39.54	0.00	3.64	24.67	856.37	56,082
Share generous Gulf War VA loans (per 100,000)	9.17	20.27	0.00	0.00	10.94	508.03	56,082
Approval rate conventional loans (in %)	72.52	15.28	0.00	63.89	84.07	100.00	56,038
Mean loan amount, conventional loans (in thous.)	144.34	78.19	11.16	97.87	169.5	2,075.65	56,005
Mean interest rate (in %)	6.84	1.00	3.45	6.13	7.62	11.34	37,518
House price growth (in %)	2.88	5.00	−44.81	0.25	5.37	56.42	56,082
Change in unemployment (in pp.)	−0.07	1.23	−13.60	−0.70	0.40	13.20	56,064
Income growth (in %)	3.58	3.76	−85.67	1.84	5.39	89.31	54,847
Population growth (in %)	0.70	1.59	−145.97	−0.17	1.32	35.46	56,082
Housing supply elasticity ρ	2.39	1.24	0.60	1.52	3.03	12.15	6,635
Army base (in miles)	336.60	269.86	8.80	147.33	438.25	1,437.97	2,218
Navy base (in miles)	444.74	284.68	6.57	200.06	643.90	1,190.53	2,218
Air Force base (in miles)	250.33	155.42	6.84	126.59	349.14	777.84	2,218
Marine Corps base (in miles)	655.64	315.36	6.57	410.27	918.63	1,376.61	2,218

Notes: This table reports summary statistics at the county-year level *ct*, separately for counties with at least one military base from which soldiers were deployed to the Gulf War, counties with at least one military base from which no soldiers were deployed to the Gulf War, and all other counties, corresponding to the respective descriptions in Tables 2.4.2 to 2.5.2. Loan amounts are converted to 2017 dollars using the Consumer Price Index Retroactive Series.

Table 2.A.2. Effect of VA Loans on House Price Growth: Robustness

Dependent variable:	House price growth$_{c,t}$		
	(1)	(2)	(3)
VA loans$_{c,t-1}$	191.2***	205.5***	163.1***
	(41.4)	(52.4)	(33.9)
log(Distance to closest non-Gulf War base)$_c \times$ Take-up rate$_t^{\text{non-Gulf War}}$			-552.1***
			(98.2)
County-Decade FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Year FE $\times$ log(Distance to closest non-Gulf War base)	No	Yes	No
Local macroeconomic conditions	Yes	Yes	Yes
Observations	58,400	58,400	58,400
Adjusted R^2	0.18	0.17	0.25

Notes: The sample is a county-year panel *ct* from 1991 to 2017. The Table reports estimates of (2.5.2) with different ways of controlling for the effect of non-Gulf War bases on house prices. In column 1, we do not control for this potentially confounding effect. In column 2, we include year-specific coefficients for the log distance to the closest non-Gulf War base. In column 3, we interact this distance with the non-Gulf War take-up rate, which is based on generous VA loans of non-Gulf War veterans. The dependent variable is the one-year house price growth rate from year t to $t-1$ in %. VA loans$_{c,t-1}$ is the relative incidence of generous VA loans. Local macroeconomic conditions include the change in unemployment rates, income growth, and population growth at the county-year level. Local mortgage market conditions include the numbers of conventional loans issued and conventional-loan applications denied per capita, as well as the number of denied applications for FHA loans per capita in county c in the previous year $t-1$. Robust standard errors, clustered at the county level, are in parentheses.

Table 2.A.3. Effect of House Price Growth on Approval Rates in the Conventional Loan Market: Restricted Samples

Sample	No VA loan issued in this year			No VA application in whole sample period		
Dependent variable:	Application approved					
	(1)	(2)	(3)	(4)	(5)	(6)
House price growth$_{c,t}$	0.019***	0.009***	0.007***	0.018***	0.008***	0.007***
	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
County-Decade FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
Lender FE	No	Yes	No	No	Yes	No
Lender-Year FE	No	No	Yes	No	No	Yes
Local macroeconomic conditions	Yes	Yes	Yes	Yes	Yes	Yes
Applicant characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	30,381,326	30,381,326	30,381,326	15,306,516	15,306,516	15,306,516
Adjusted R^2	0.15	0.32	0.35	0.14	0.32	0.35

Notes: In columns 1 to 3, the sample consists of all mortgage applications in the conventional loan market at the transaction level m from 1991 to 2017 to lenders that did not originate a VA loan in the same year. In columns 4 to 6, we further restrict the sample to applications to lenders that did not receive an application for a VA loan during the entire sample period. The table reports IV estimates of (2.5.8). The dependent variable is a dummy for whether the application was granted. The endogenous variable is the one-year house price growth rate from year t to $t-1$ in %. Local macroeconomic conditions include the change in unemployment rates, income growth, population growth, and the product of the log distance to the closest military base from which no soldiers were deployed to the Gulf War times the Gulf War take-up rate at the county-year level. Applicant characteristics include a dummy for white applicants, a dummy for male applicants, and the log income of the applicant. Robust standard errors, clustered at the county level, are in parentheses.

Table 2.A.4. Summary Statistics: Conventional Mortgages by Owner-occupied Status

		Mean	SD	Min	P25	P75	Max	N
Not owner-occupied	Application approved	0.8	0.4	0.0	1.0	1.0	1.0	10,418,551
	Loan amount (in thous.)	189.5	268.3	0.0	75.4	234.0	15,9637.4	10,418,543
	Applicant income (in thous.)	194.0	322.8	1.0	81.0	212.0	180,000.0	10,418,551
	Loan-to-income	1.4	4.8	0.0	0.6	1.7	5,000.0	10,418,543
Owner-occupied	Application approved	0.8	0.4	0.0	1.0	1.0	1.0	76,603,362
	Loan amount (in thous.)	209.3	256.7	0.0	85.3	269.1	309,000.0	76,603,226
	Applicant income (in thous.)	109.8	195.4	1.0	53.1	126.5	542,821.0	76,603,362
	Loan-to-income	2.2	5.5	0.0	1.3	2.8	19,618.0	76,603,226

Notes: The table reports summary statistics for the universe of loan applications in the conventional loan market in the HMDA data at the application level m, separately for applications where the applicants will and will not occupy the home for which they take out the mortgage, as used in Table 2.5.4. All dollar values are converted to 2017 dollars using the Consumer Price Index Retroactive Series.

Appendix 2.B List of Military Bases

In this section, we describe how we construct Table 2.B.1, a list of U.S. military bases from which soldiers were deployed during the Gulf War. The U.S. military includes four branches: the Army, the Navy, the Air Force, and the Marine Corps.[25] First, we hand-collect a list of all units that served in operations Desert Shield and Desert Storm.[26] Next, we match all units to their home bases, which gives us the required list of all bases. Finally, we retrieve the coordinates of those bases.[27] In particular, we use five sources that provide information on U.S. military units involved in these operations. Below, we describe these sources, how we extract the deployed units and their corresponding home bases, and how we assign coordinates to these bases.

2.B.1 Sources

We gather information from five sources. Four of them are official documents, namely the Association of the United States Army's Special Report (West and Byrne, 1991), the Department of the Navy's Summary Report (Chief of Naval Operations, 1991), the study by Cohen (1993) on the U.S. Air Force, and the publication by Westermeyer (2014) on the U.S. Marine Corps. Finally, we use the private website desert-storm.com.

2.B.2 Compiling a List of Units and Bases

Army report. Two tables on pages 7 and 8 of West and Byrne (1991)'s Special Report list the U.S. Army units deployed during Operation Desert Shield. The tables cover each of the two phases of deployment during the operation. From these tables, we extract the names of the units and their respective home bases. This results in a list of 14 Army units with nine bases in the United States.

Navy report. Pages B-1 through B-9 of Chief of Naval Operations (1991)'s Appendix B list the participating naval units. However, the report does not provide the respective home ports. Thus, we use the Navy report to corroborate information about naval units available from other sources, particularly desert-storm.com. This results in a loss of information if some naval units do not appear in the other sources. However, with this report we can already confirm the deployment of 102 U.S. Navy units that we extracted from desert-storm.com.

25. We exclude National Guard and Reserve forces from our considerations. Although they are generally eligible for VA loans, our data show that few VA loans were issued to veterans who had served in the National Guard or Reserve forces.

26. Note that all military branches are further organized in entities, e.g., Corps, Divisions, etc. For pragmatic reasons, we adhere to the level of granularity provided by each of the sources we analyze, as a result of which we use the same term "unit" across different military entities.

27. We disregard units with home bases outside the U.S.

Table 2.B.1. List of Military Bases

#	Branch	Base name	lat	lon
1	Air Force	Bergstrom AFB	30.17630	-97.67209
2	Air Force	Davis-Monthan AFB	32.16080	-110.84872
3	Air Force	Eglin AFB	30.57594	-86.52837
4	Air Force	England AFB	31.33467	-92.54034
5	Air Force	George AFB	34.58522	-117.37325
6	Air Force	Griffiss AFB	43.23000	-75.41000
7	Air Force	Hill AFB	41.12808	-111.99125
8	Air Force	Hurlburt	30.42919	-86.69871
9	Air Force	Langley AFB	37.08557	-76.36437
10	Air Force	Little Rock AFB	34.90400	-92.13847
11	Air Force	Loring AFB	46.94972	-67.88889
12	Air Force	Moody AFB	30.97253	-83.16469
13	Air Force	Myrtle Beach	33.67972	-78.92833
14	Air Force	Pope AFB	35.17083	-79.01444
15	Air Force	Robins AFB	32.61755	-83.58158
16	Air Force	Seymour-Johnson AFB	35.34790	-77.96258
17	Air Force	Shaw AFB	33.97486	-80.47042
18	Air Force	Tinker AFB	35.41919	-97.39293
19	Air Force	Wurtsmith AFB	44.45250	-83.38028
20	Army	Fort Benning	32.39995	-84.80062
21	Army	Fort Benning	32.28387	-84.95484
22	Army	Fort Bliss	32.26208	-106.07540
23	Army	Fort Bragg	35.13624	-79.14397
24	Army	Fort Campbell	36.59649	-87.59905
25	Army	Fort Hood	31.21569	-97.73703
26	Army	Fort McPherson	33.70621	-84.43328
27	Army	Fort Riley	39.18668	-96.82087
28	Army	Fort Sill	34.68226	-98.48341
29	Army	Fort Stewart	31.99357	-81.61677
30	Marine Corps	Camp Lejeune	34.64336	-77.30510
31	Marine Corps	Camp Pendleton	33.36176	-117.42357
32	Marine Corps	Norfolk	36.94331	-76.30151
33	Navy	Bremerton	47.55559	-122.65236
34	Navy	Charleston	32.96293	-79.96357
35	Navy	Concord	38.05140	-122.01880
36	Navy	Earle	40.25386	-74.16085
37	Navy	Little Creek	37.88615	-75.46864
38	Navy	Long Beach	33.74202	-118.23341
39	Navy	Mayport	30.38159	-81.42483
40	Navy	New Orleans	29.83136	-90.02087
41	Navy	Newport	41.53528	-71.30964
42	Navy	Norfolk	36.94331	-76.30151
43	Navy	Oakland	37.78611	-122.31861
44	Navy	Pearl Harbour	21.33657	-157.94791
45	Navy	Philadelphia	39.89111	-75.17861
46	Navy	San Diego	32.67576	-117.12275

Notes: This table lists all military bases from which soldiers were deployed to the Gulf War and their location, by military branch and name.

Air Force report. The survey by Cohen (1993) is extensive in terms of the number of Air Force units listed, and includes detailed information on the corresponding home bases. In particular, pages 58 to 64 of this survey are relevant for the Air Force deployment. From the respective tables therein, we extract the participating units and their respective home bases. Note that information on some reported units is marked as “unknown.” In particular, if a unit’s home base is marked as “unknown,” we do not include it in our list. This results in 79 units deployed from 52 Air Force bases.[28]

28. We noticed that some of these bases may have actually been Air National Guard bases, which we disregard in our analysis. However, these cases are not relevant for our estimations as they only appear in one of our sources. As we describe below, we use only those bases that appear in at least two of our sources. Thus, the fact that some Air National Guard bases are present in our sample does not affect our results.

The Air Force report also includes data on participating Army and Navy units, but without reference to their home bases. Thus, we use this information only to corroborate information on deployed units from other sources.

Marine Corps report. Appendix A, i.e., pages 241 to 250, in Westermeyer (2014) lays out in detail the involvement of the Marine Corps in the Gulf War. However, it only lists units without their home bases. Therefore, we proceed analogously to the Navy report, and use this source to corroborate the information on Marine Corps units available from desert-storm.com. As a result, we are able to confirm six of the ten Marine Corps units listed on the aforementioned website and three unique bases.[29]

desert-storm.com. desert-storm.com is a private website, which, according to its own disclosure, was initiated by a student in 1997 to collect information about the Gulf War operations, make it available to the public, and support veterans of the war. We use the URL desert-storm.com/soldiers/units.html and the subsequent links therein.[30] The site provides lists of Army, Navy, Air Force, and Marine Corps units.

From these lists, we extract all Army, Navy, Air Force, and Marine Corps units that were deployed from a base in the United States. In all but two cases, the website lists the home bases of the units.[31] This procedure yields a large number of units assigned to bases. Specifically, the total includes 196 units. These units are assigned to 61 unique bases. There is a large overlap between the bases gathered from this unofficial source and those obtained from the official sources.

2.B.3 Assigning Coordinates to Bases

Finally, we assign coordinates to the 94 unique bases involved in the Gulf War deployment. To do this, we rely on the National Transportation Atlas Database, published by the U.S. Department of Transportation in 2019: "The dataset depicts the authoritative boundaries of the most commonly known Department of Defense (DoD) sites, installations, ranges, and training areas in the United States and Territories. (...) Sites were selected from the 2010 Base Structure Report." We attain the list from public.opendatasoft.com/explore/dataset/military-bases/table/. It contains the coordinates of the bases.[32] We hand-match our list of Gulf War bases to this list using the name of the site and the military branch.

Around 22% of the bases in our list do not appear in this official dataset. In many cases, this is due to base closures in the period from the Gulf War to 2010, when the Base Structure Report was published. In these cases, whenever possible,

29. One of these bases is also a Navy base, namely Norfolk, Virginia.
30. Last retrieved on December 14, 2022.
31. Units for which the home bases were unknown are omitted.
32. Last retrieved on December 14, 2022.

we obtain the coordinates from a manual web search, mainly using Wikipedia and Google Maps. With this approach, we match 15 more bases to exact locations. For the remaining six bases we were unable to find any coordinates.

2.B.4 Quality Assurance

Since we cannot match all Navy and Marine Corps units with home bases through official reports, and to ensure data quality, we only use those bases that we find in at least two of our five sources.[33]

33. Note that with this approach, only two bases which we would like to use in our estimations could not be matched to coordinates.

Appendix 2.C Local Government Spending

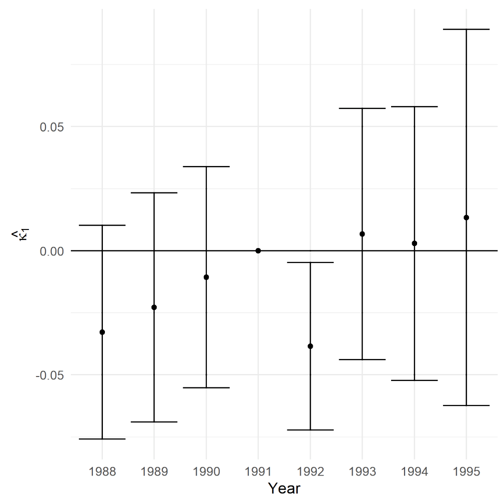
To rule out the possibility that house prices in counties around Gulf War bases rise due to increased local government spending in counties with bases from which soldiers were deployed to the Gulf War, we analyze county-level finance data compiled by Pierson, Hand, and Thompson (2015). We construct a balanced county-year sample from 1988 to 1995 and exclude counties that are not in our sample. To examine the behavior of local spending around the Gulf War, we estimate the following regression specification:

$$\begin{aligned}\log(\text{Total expenditure})_{c,t} =& \kappa_\psi \mathbb{1}_{t=\psi} \times \mathbb{1}_{\text{Gulf War Base in } c} \\ &+ \kappa_2 \log(\text{Total revenue})_{c,t-1} + \iota_c + \chi_t + \varepsilon_{c,t}, \quad (2.C.1)\end{aligned}$$

where Total expenditure$_{c,t}$ is the total expenditure of county c in year t in thousands of nominal dollars. $\mathbb{1}_{\text{Gulf War base in } c}$ is 1 if the midpoint of at least one of the Gulf War bases listed in Table 2.B.1 is within the county's boundaries as of 1990. We choose 1991 as the reference year for $\mathbb{1}_{t=\psi}$. Thus, κ_ψ gives the differential effect of expenditure between bases with and without bases in year ψ. We control for lagged total revenue as well as county and year fixed effects.

Figure 2.C.1 shows the results. We find no evidence of differential local government spending around the Gulf War between counties with and without a Gulf War base.

Figure 2.C.1. Local Government Spending in Counties with and without Gulf War Bases around the Gulf War



Notes: The dots in this figure are the point estimates for κ_ψ in equation (2.C.1), i.e., the year-specific differential effect of local government spending between counties with and without a Gulf War base, for $t \in \{1988, 1989, ..., 1995\}$. Bars represent 95% confidence intervals. Robust standard errors are clustered at the county level.

Appendix 2.D Short-Term Anticipation Effect

To evaluate the possibility that conventional mortgagors might anticipate the future take-up of VA loans by veterans, and the associated house price growth, we exploit the empirical fact (see Figure 2.4.1 in the main paper) that the American-led invasion of Afghanistan in 2001 and the subsequent Iraq War starting in 2003 was associated with differential timing of the take-up of VA loans across military branches. While Army veterans exhibit a steep increase of take-up rates around the conflict years, our previous assumption that take-up materializes over time holds for all other military branches.

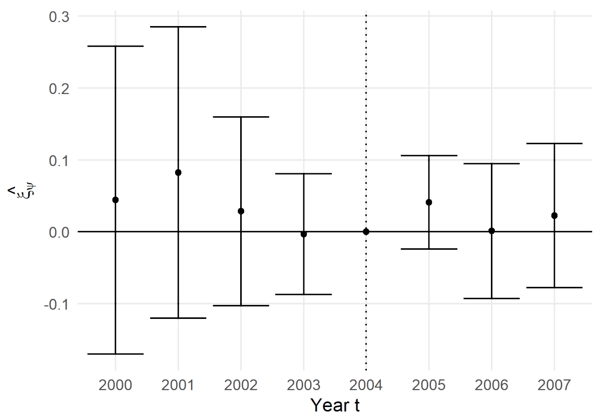
Building on the idea that the conventional loan market reacts to beliefs regarding house price growth, and that the latter are affected by the return of deployed veterans who are thereafter entitled to VA housing benefits, we estimate the following regression for the subsample from 2000 to 2007 for counties within 50 miles of a Gulf War base. We define a county as close to an Army base if at least one Army base from which soldiers were deployed to the Gulf War is within 50 miles:

$$\begin{aligned}\log(\text{Applications in the conventional mortgage market})_{c,t} =& \xi_\psi \mathbb{1}_{t=\psi} \times \mathbb{1}_{\text{Close to Army base } c} \\ &+ \iota_c + \chi_t + \varepsilon_{c,t},\end{aligned} \tag{2.D.1}$$

where ι_c and χ_t denote county and year fixed effects, respectively.

Figure 2.D.1 shows the results. We find no evidence of significant differential anticipation effects at the county level.

Figure 2.D.1. Conventional Mortgage Applications in Counties Close to Army Gulf War Bases around the Iraq War



Notes: The dots in this figure are the point estimates for ξ_ψ in equation (2.D.1), i.e., the year-specific differential effect of the logged number of applications in the conventional mortgage market between counties near Army Gulf War bases and counties near other Gulf War bases, for $t \in \{2000, 2002, ..., 2007\}$. Bars represent 95% confidence intervals. Robust standard errors are clustered at the county level.

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Chapter 3

Monetary Policy, Macroprudential Policy, and Corporate Lending Rates in the Euro Area

Joint with Jan-Hannes Lang and Marek Rusnák

3.1 Introduction

Monetary and macroprudential policies are typically administered by the same institutions - central banks. Conceptually, both policy types are often tightened during periods of economic expansion and rising inflationary pressures. While tighter monetary policy and higher macroprudential capital buffer requirements can both dampen bank credit supply, the relative influence of macroprudential measures remains uncertain, as they have only recently become a key element of the policy toolkit. A notable example of simultaneous monetary and macroprudential tightening is the recent experience in the euro area, where the European Central Bank (ECB) raised its deposit facility rate from –0.5% to 4% between spring 2022 and autumn 2023, while several national authorities simultaneously increased their countercyclical capital buffer (CCyB) rates (see Figure 3.1.1). During this period, bank loan growth decelerated sharply, lending rates rose significantly, and banks reported a sustained tightening of credit standards. Against this background, this paper examines the relative impact of monetary and macroprudential policy on bank credit supply, using lending rates as a measure for the intensive margin.

We first establish that monetary policy is the predominant driver of bank lending rates, exerting a substantially larger effect than macroprudential capital buffer

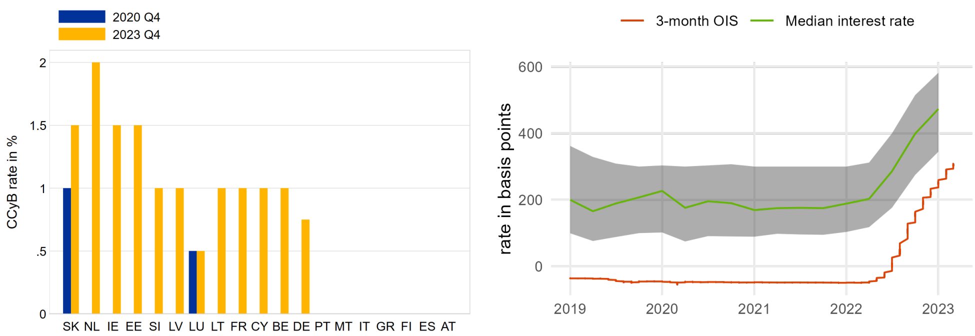
* Any views expressed are only those of the authors and do not necessarily represent the views of the ECB, Deutsche Bundesbank, or the Eurosystem.

requirements. We then identify three conditions under which the effects of the two policies converge to some extent. First, along the time dimension, we show that monetary policy transmission weakens as policy rates approach the zero lower bound, reducing the relative importance of monetary policy. Second, using cross-country variation, we find that in financial systems where the corporate bond market—an alternative funding source for firms—is less developed, banks have a greater ability to pass through funding costs, reducing the dominance of monetary policy. Third, along the bank dimension, we obtain mixed results. Higher levels of capital imply a lower dominance of monetary relative to macroprudential policy. This also holds for higher capital headroom, i.e. capital above regulatory requirements - but only across banks.

To establish these results, we test and confirm the predictions of a simple theoretical framework of bank lending rates using a granular, confidential data set covering the universe of corporate loans in the euro area in combination with supervisory bank data. This data set allows us to study the impact of monetary policy and macroprudential policy on lending rates while holding firm credit demand and default risk constant, which might both change endogenously in response to monetary and macroprudential policy. Moreover, our dataset enables a comprehensive analysis across a diverse set of countries and over time.

More precisely, we estimate the impact of monetary and macroprudential policy on interest rates for new loans to euro area non-financial corporations over the period 2019 to 2023. Our analysis leverages the euro area corporate credit register (Anacredit), which we merge with bank-level supervisory data and macroeconomic variables, to estimate loan pricing regressions on about 14 million new loans from

Figure 3.1.1. Countercyclical Capital Buffer Rates and Interest Rates



(a) Announced National Countercyclical Capital Buffer Rates

(b) OIS and Unconditional Loan Rates

Notes: For all euro area countries, the left panel of this figure shows the announced national countercyclical capital buffer rates in 2020 Q4 (blue) and 2023 Q4 (yellow) in percent. The red line in the right panel shows the 3-month OIS rate for each business day. The green line shows the median interest rate of all new loans issued by euro area banks to non-financial firms and the grey area the corresponding interquartile range.

15 euro area countries. This data allows us to control for various time-varying bank-level, firm-bank-level, and country-level observable as well as unobservable heterogeneity with control variables and fixed effects to isolate the impact on bank loan supply that we are interested in. In particular, firm-quarter fixed effects account for time-varying credit demand at the firm level which allows us to study credit supply effects. As our measure of monetary policy we use the daily 3-month overnight indexed swap (OIS) rate, which is a short-term risk-free interest rate that follows key ECB monetary policy interest rates closely. In doing so, we exploit the intra-quarter variation in monetary policy. The identification of β then relies on the assumption that firms do not systematically adjust their credit demand to monetary policy within a quarter and that monetary policy does not affect other macroeconomic conditions that simultaneously affect bank lending, beyond the variables we control for.

As our measure of macroprudential policy, we use the bank capital-to-asset ratio, which is also used for example in Santos and Winton (2019), and refer to this measure as bank capitalization from now on. The rationale for this choice is that the impact of higher macroprudential capital buffer requirements on lending rates should ultimately depend on how much additional equity funding per unit of asset the bank decides to use. This, in turn, depends on how much the bank's capital ratio target increases in response to higher capital buffer requirements - typically less than one-for-one (Couaillier, 2021) - and on the average risk-weight, as macroprudential buffers typically apply to risk-weighted capital ratios. Using bank capitalization rather than risk-weighted capital buffer requirement captures both of these channels and is therefore a good measure for the impact of higher macroprudential capital buffer requirements on lending rates. To account for potential confounding factors that might be correlated with bank capitalization and lending rates, we use bank-firm fixed effects and a set of time-varying bank characteristics. To identify the effect of bank capitalization, we rely on the assumption that there are no bank characteristics that vary over time, are correlated with bank capitalization and affect lending rates, other than our control variables. Notably, we do not analyze the effects of borrower-based macroprudential measures, as these are primarily used for retail rather than corporate lending (Peydró, Rodriguez-Tous, Tripathy, and Uluc, 2023; van Bekkum, Gabarro, Irani, and Peydró, 2024).

Our first key finding is that monetary policy has a substantially higher impact on bank lending rates than macroprudential policy. The estimates of our most stringent specification imply that even the smallest monetary policy change of 25bps would require an increase in macroprudential buffer requirements of about 2.4 pp to have the same effect on bank lending rates. This would be a large change in the macroprudential stance, as Figure 3.1.1a shows that, for example, CCyB rates in all countries were not larger than 2% in all countries as of Q4 2023 - and this is only for a small monetary policy change of 25bps.

Our second set of results documents the economic circumstances under which the effects of monetary policy and bank capitalization converge to some extent. In particular, we show that the level of policy rates matters: At the zero lower bound, the relative importance of monetary policy is reduced by about one-half. We also document a substantial impact of the variation in the importance of the corporate bond market: A one standard deviation higher importance reduces the monetary policy pass-through by 9.5% and the impact of bank capitalization by even 38%. Finally, we explore the importance of the differences in bank capitalization: In particular, a one standard deviation higher capitalization reduces the monetary policy pass-through by 7.2% and the impact of bank capitalization itself by only 1.7%.

We additionally show the results of two exercises to present the robustness of our results. First, similar to Bu, Rogers, and Wu (2021), Döttling and Ratnovski (2023), and Elliott, Meisenzahl, and Peydró (2024), we employ an IV estimation strategy, where we instrument the level of monetary policy with the cumulative sum of high-frequency monetary policy surprises provided by Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019). Using only this exogenous component of monetary policy, we find somewhat lower pass-through coefficients, which, however, still implies a dominance of monetary relative to macroprudential policy. Second, we explore the role of loan terms, such as maturity and collateralization, which could be affected by monetary and macroprudential policy. We show that loan terms do not explain our results.

The estimated magnitudes for the impact of monetary policy and macroprudential policy (bank capitalization) on bank lending rates that we find in our empirical study are consistent with the implications of a simple theoretical framework for bank lending rates. In the theoretical framework, lending rates are determined by bank funding costs and a mark-up. Bank funding costs are, in turn, determined by monetary policy interest rates, the bank capital-to-asset ratio, and the equity premium, i.e., the difference between the cost of bank equity and bank debt. Within this theoretical framework, a 1 pp increase in the monetary policy interest rate should increase bank lending rates by 70 bps, assuming that monetary policy pass-through to bank debt funding costs is 70%. Moreover, for a bank equity premium of 12%, a 1 pp increase in the capital-to-asset ratio should increase bank lending rates by 12 bps. Both magnitudes are similar to our empirical estimates and in line with what the existing literature implies.

Our findings have important policy implications. First, monetary policy tightening was the major driver behind the recent increases in bank lending rates and tightening of bank credit supply observed in the euro area, while increases in macroprudential capital buffer requirements have played a limited role. Second, given the comfortable capital headroom of euro-area banks and solid profitability, which allows banks to increase capital without issuing new equity, a potential further tightening of macroprudential buffer requirements is unlikely to have a big negative impact on bank loan supply. Third, a potential release of macroprudential buffer requirements at the current juncture where banks have ample capital headroom would not have a material impact on bank loan supply and lending rates and, therefore, would not help to boost investment and economic growth.

Related literature. Our paper contributes to three strands of the literature. First, it relates to the literature on the pass-through of monetary policy to bank lending rates. Most studies on this topic use country-level or bank-level data (Gregor, Melecký, and Melecký, 2021; Beyer, Chen, Misch, Li, Ozturk, et al., 2024). The estimated pass-through is typically found to be incomplete because of imperfect information and competition and varies depending on many factors such as borrower characteristics, maturity, credit risk as well as bank characteristics such as size and asset quality (Gambacorta, Illes, and Lombardi, 2015; Andries and Billon, 2016; Holton and Rodriguez d'Acri, 2018; Gregor, Melecký, and Melecký, 2021; Beyer et al., 2024). We contribute to the literature by estimating the impact of monetary policy at the individual loan level. In addition, we document that the pass-through is lower at the zero lower bound, in countries where the corporate bond market is more important, and for banks with more capital and more capital headroom. While the calibrated model of Abadi, Brunnermeier, and Koby (2023) implies that the pass-through of monetary policy could be lower near the zero lower bound, the empirical evidence on this is scarce to our knowledge. Holm-Hadulla and Thürwächter (2021) show that the impact of monetary policy on credit costs is lower if the bond share is higher. However, their results are for the overall cost of credit. Finally, relative to the existing literature, we contribute by evaluating and comparing the effects of monetary and macroprudential policy in a joint specification. Altavilla, Laeven, and Peydró (2020) study the complementarities of monetary and macroprudential policy but use only variation in macroprudential policy at the country level and do not compare the relative importance of the two policies.

Second, our paper contributes to the literature studying the impact of bank capital on lending and lending rates. The estimates reported in the literature are generally small and inconclusive: the estimated impact of 1pp increase in the capital-to-asset ratio of banks on lending rates ranges between -0.1pp to 0.3pp based on the 41 standardised estimates[1] from 16 studies published until 2019 that are available in the BIS FRAME repository (Boissay, Cantu, Claessens, and Villegas, 2019).[2] Most of the literature studying the impact of bank capital on lending rates uses country-level or bank-level data (Boissay et al., 2019), but there is relatively little evidence using loan-level data.[3] By using the granular loan level data, we provide evidence that

1. This includes both estimates using NFC and HH lending rates.

2. Similarly, the most recent empirical studies also do not provide conclusive evidence: several studies do not find a significant effect of bank capital or bank capital requirements on lending rates (Imbierowicz, Löffler, and Vogel, 2021; Ehrenbergerová, Hodula, and Gric, 2022), while others find positive but small effect (Glancy and Kurtzman, 2021; Bichsel, Lambertini, Mukherjee, and Wunderli, 2022).

3. Santos and Winton (2019) use granular data on pre-2007 US publicly traded firms from mostly syndicated loans and find that higher bank capital has a negative impact on loan rates. Glancy

higher bank capital ratios do exert upward pressure on lending rates, in particular when studying differences in bank capitalization over time within the same bank, i.e., with bank fixed effects. In addition, we show that the impact is weaker at the zero lower bound, in countries where the corporate bond market is more important and when banks' capitalization is further away from regulatory requirements.

Third, by comparing the impact of monetary and macroprudential policy, we contribute to a rather new literature that studies the effects of macroprudential policy. Peydró et al. (2023) and van Bekkum et al. (2024) study borrower-based macroprudential policies while we study the impact of capital-based policies. Couaillier and Henricot (2023) study market reactions to macroprudential policy announcemeents.

The remainder of this paper is structured as follows. In section 3.2 we describe a simple theoretical framework for bank lending rates that helps to put structure on the empirical analysis and allows for a better interpretation of estimated results. In section 3.3 we describe the dataset for our empirical analysis. Section 3.4 then presents our empirical strategy and the main empirical results regarding the impact of monetary policy and macroprudential policy on bank lending rates. Section 3.5 shows the results of two robustness exercises. Finally, section 3.6 concludes.

3.2 A theoretical framework for bank lending rates

To guide our empirical analysis, we develop a simple theoretical framework of how monetary policy and bank capitalization affect the pricing of bank loans. Such a model framework helps to structure the empirical analysis and allows for a better interpretation of the estimated results.

Consider a bank that provides loans L and finances these with debt D and equity E so that the balance sheet identity is $L = D + E$. Bank capitalization is measured by the leverage ratio, which is defined as the ratio of equity over total loans $LR = E/L$. Hence, the balance sheet identity can be rewritten as $D/L = 1 - LR$.

The bank pays an interest rate i^D on its debt, and bank equity is assumed to always be more costly than debt by a constant equity premium ρ so that $i^E = i^D + \tilde{\rho}$. Furthermore, assume that the cost of bank debt moves in line with the monetary policy interest rate set by the central bank i^{CB} according to a constant pass-through parameter $\beta \in [0; 1]$ so that we can write $i^D = \beta \cdot i^{CB}$.

Finally, assume that bank funding costs are passed on one-for-one to bank lending rates and that banks charge a constant markup μ over their funding costs to cover

and Kurtzman (2021) use loan-level data covering US commercial real estate firms collected in the context of Comprehensive Capital Analysis and Review and find that a 1pp increase in capital requirements increases loan rates by 8.5 basis points. Jaunius Karmelavičius and Buteikis (2023) use granular data on lending by Lithuanian banks and find that capital requirements may have elevated lending rates by only 0.1 pp on average, but the primary driver behind the interest rate changes during 2015-2019 was market concentration.

operating and other expenses and as compensation for credit risk. Hence, lending rates can be expressed as $i^L = \mu + i^D \cdot D/L + i^E \cdot E/L$. If we make use of the balance sheet identity, the definition of the leverage ratio, and the definitions of debt and equity funding costs, this can be rewritten as:

$$i^L \;=\; \mu + \beta i^{CB} \cdot (1 - LR) + (\beta i^{CB} + \tilde{\rho}) \cdot LR \tag{3.2.1}$$

As the markup μ, the monetary policy pass-through β, and the equity premium $\tilde{\rho}$ are all assumed to be constant over time, changes in bank lending rates will be driven by changes in monetary policy interest rates i^{CB} and changes in bank capitalization as measured by the leverage ratio LR. If we rearrange equation (3.2.1), we get the following expression for bank lending rates:

$$i^L \;=\; \mu + \beta \cdot i^{CB} + \tilde{\rho} \cdot LR \tag{3.2.2}$$

Hence, for a pass-through coefficient of $\beta = 1$, a 100 bps increase in the monetary policy rate should increase bank lending rates by 100 bps. If the pass-through coefficient were 0.70, the same monetary policy tightening impulse would lead to an increase in lending rates of 70 bps. Moreover, for a bank equity premium $\tilde{\rho}$ of 12%, a 100 bps increase in the leverage ratio should increase bank lending rates by 12 bps.[4] These indicative magnitudes will be helpful to benchmark our empirical results in the remainder of this paper.

Until now, we have imposed that β and $\tilde{\rho}$ do not vary over time, across countries, and across banks. We will relax these assumptions in the second part of our empirical analysis.

3.3 Data

Our empirical analysis relies primarily on loan-level data from the euro area credit registry (AnaCredit), which covers the universe of all corporate loans and provides detailed information on loan and borrower characteristics. We combine this dataset with confidential supervisory bank information and macroeconomic data. Next, we describe the different data sources.

3.3.1 Loan-level Microdata

AnaCredit (Analytical Credit Datasets), a comprehensive and confidential database maintained by the European Central Bank (ECB), was launched in September

4. Altavilla, Bochmann, De Ryck, Dumitru, Grodzicki, et al. (2021) provide comprehensive evidence on the equity premium for euro area banks. The self-reported equity premia of banks are between 8% and 12%. Model-average estimates for the cost of equity show substantial variation: At the end of 2019, the estimates for the equity premium ranged from 9.2% at the 10th percentile to 15.7% at the 90th percentile.

2018. It harmonizes various national credit registers across euro-area countries. The database contains detailed loan-level information on all loans to legal entities with a reporting threshold of €25,000. The harmonization ensures consistency and comparability of credit data across different jurisdictions within the euro area.

We obtain loan-level micro data on loans issued by banks in all 19 euro area countries to euro area non-financial firms for the first quarter of 2019Q1 to the first quarter of 2023. Only very small banks are not included in our sample. While they report to the national central banks, their data is not forwarded to the European Central Bank for the euro area AnaCredit Europe.

Our analysis is based only on new loans to non-financial corporations located in a euro area country. We exclude borrowers in NACE sectors 64-66, which are financial services. In total, our sample contains 14 million individual loan issuances. The microdata on these loans provides detailed information on the borrower, such as the probability of default (PD) estimate of the bank. Furthermore, it contains detailed loan characteristics such as loan amount, maturity, instrument type, amortization scheme, recourse, interest rate type, and collateral.

We impute missing PDs, where possible. Only banks that use internal models to calculate risk weights to calibrate capital requirements are required to report the PD of borrowers. If the PD is not reported for a loan, we impute the PD using the median PD reported for all loans to the same borrower in the same month by other banks. We drop the loan if this information is unavailable because the borrower only received new loans from banks that do not report PDs. Since this procedure leaves us almost no loans for Cyprus, Estonia, Malta, and Slovenia, we drop these countries from our sample.

We apply some additional filters to our raw sample. First, we exclude loans under government COVID-19 support programs and loans with a guarantee where the guarantor belongs to any level of government because, for these loans, we expect government involvement to significantly affect the relationship between monetary policy, bank capitalization, and interest rates. Second, we exclude loans with no outstanding amount and loans for which any of our key variables are missing. This includes loans issued by banks that are not supervised by the Single Supervisory Mechanism (SSM) as either Significant or Less Significant Institutions for which we do not have bank information. Finally, we exclude loans with an interest rate below 0 or above 20% as these are likely to be reporting errors. Furthermore, we exclude some instrument types: Deposits, repos, overdrafts, credit cards, and missing. We restrict our sample to euro-denominated loans and loans with only one debtor and one creditor.[5] We exclude loans for which the interest rate is missing, and either the borrower or the instrument is classified as in default at the time of origination. We

5. Note that this does not exclude syndicated loans, which are reported as multiple loans.

Table 3.3.1. Summary Statistics: Loan and Borrower Characteristics

	Mean	SD	Min	P25	Median	P75	Max	N
Interest rate	251.0	198.5	0.0	100.5	216.0	347.2	2,000.0	14,374,070
PD	2.5	4.6	0.0	0.5	1.3	2.3	51.3	14,374,070
Loan amount	67.8	362.8	0.0	4	17.2	37.9	122,415.3	14,374,070

Notes: The table reports summary statistics for new loans issued by euro area banks to non-financial firms between Q1 2019 and Q1 2023. *Interest rate*, measured in basis points, is a loan's interest rate on the issuance day. *PD* is the bank's estimate for the probability that the borrower defaults within the next year in percent. *Loan amount* is given in thousands.

restrict the sample to loans issued by banks, thus excluding other financial institutions that can also be present in AnaCredit.

AnaCredit allows for several ways to define *new* loans. Ultimately, we are interested in how monetary policy and bank capitalization affect the interest rate on the day the contract is signed. However, banks are only required to report a loan when money is first drawn. Some time may elapse between the signing of the contract and the drawing of money, particularly for credit lines. We use the date the contract is signed ("inception date") as the relevant date to merge other variables. In order to rule out that the loan terms have changed between the date the contract is signed and the date we first observe the contract, we remove all loans that are reported for the first time more than two months after issuance.

We have to deal with outliers for the outstanding amount and the PD. We winsorize the upper end of the outstanding amount at the 99% level. We exclude 1% of the highest PDs.

In AnaCredit, each financial instrument is reported separately, and a contract may contain several financial instruments. However, the same borrower and debtor often agree on multiple instruments with identical conditions within one contract. Whenever this happens, we collapse all instruments with identical characteristics within one loan contract into one observation and aggregate the loan amount. This reduces the sample by about 23%.

Table 3.3.1 reports descriptive statistics at the loan level, particularly for our key variable of interest, the loan's interest rate at issuance. The average (median) loan is issued at an interest rate of 253bps (214bps). However, there is wide variation in our dependent variable: The 25th percentile is issued at 100bps while the 75th percentile is issued at 351bps.

3.3.2 Bank-level Data

We combine the loan-level microdata with supervisory bank information for the quarter preceding the quarter in which the loan was issued. For banking groups, we always refer to the highest consolidation level within the euro area as macroprudential policies affect capital requirements at the group level. We use the bank capital-

Table 3.3.2. Summary Statistics: Bank Characteristics

	Mean	SD	Min	P25	Median	P75	Max	N
Bank capital	7.7	2.5	−2.3	6	7.2	9.1	94.4	14,374,070
Capital headroom	3.1	1.9	0.0	1.7	2.9	4.3	8.6	14,373,703
Total assets	693.3	737.1	0.0	56.7	335.7	1,492.3	2,766.4	14,374,070
Return on assets	0.6	0.6	−16.2	0.4	0.5	0.9	5.9	14,374,070
NPL ratio	3.4	2.7	0.0	1.8	2.7	3.8	75.5	14,374,070
Provisioning ratio	0.7	0.7	−6.0	0.3	0.5	0.9	26.3	14,325,119

Notes: The table reports summary statistics for the bank characteristics for new loans issued by euro area banks to non-financial firms between Q1 2019 and Q1 2023. Bank characteristics are lagged by one quarter. *Bank capital*$_{b,q-1}$ is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}_{b,q-1}$. *Capital headroom*$_{b,q-1}$ is the distance between the actual capital ratio and the regulatory requirement in percentage points. *Total assets* are in billion. The *return on assets*, the *NPL ratio*, and the *provisioning ratio* are all in percent.

to-asset ratio, which is also used for example in Santos and Winton (2019), as our measure of macroprudential policy, and we refer to this measure as bank capitalization, which we will denote by *Bank capital* in our tables. The main reason for using bank capitalization in our regressions is that the effect of higher macroprudential capital buffer requirements on lending rates should ultimately depend on how much additional equity funding per unit of asset the bank decides to use. This, in turn, depends on how much the bank's capital ratio target increases in response to higher capital buffer requirements, typically less than one-for-one (Couaillier, 2021), and on the average risk-weight of the bank. Using bank capitalization in our regressions instead of the risk-weighted capital buffer requirement accounts for both of these channels and is, therefore, a good proxy for the impact of higher macroprudential capital buffer requirements on lending rates.

In the second part of our empirical analyses, we will also use the capital headroom of banks. The capital headroom is defined as the ratio of the core equity tier 1 capital (CET1) that the bank has above all regulatory requirements, relative to its risk-weighted assets (RWA) including pillar 2 guidance (P2G).[6] It measures how much capital the bank can deplete before supervisors impose consequences such as limits on dividend payments. As control variables, we use a bank's total assets as a measure of its size and its return on assets as a measure of profitability[7], its NPL ratio, and the provisioning ratio. We winsorize the capital headroom at the 1% and 99% levels.

Table 3.3.2 reports descriptive statistics of the bank characteristics at the loan level. The average (median) loan is issued by a bank with *Bank capital* of 7.6%

6. Banks have to meet several regulatory requirements simultaneously: The CET1 requirement can only be met with CET1 capital. However, the Tier 1, Tier 2, and Leverage Ratio requirements can, but do not have to be met with CET1 capital.

7. We measure profitability by the return on assets. If values for the return on assets are missing, we interpolate linearly if values are not missing for more than three consecutive quarters.

(7.0%). In other words, the average issuing bank is funded with more than 90 % of debt. Observations in the interquartile range are relatively concentrated between 6.0 and 8.7%, but overall, there is a lot of variation. Over time, the ratio is rather persistent over the four years of our sample: For the average (median) bank, the difference between the highest and the lowest value for the capital-to-assets ratio is only 2.0 (1.4) percentage points. On average, banks have a *Capital headroom* of 3.2 percentage points relative to RWAs above their regulatory requirements. However, there is also wide variation; some banks are close to their regulatory requirements.

3.3.3 Macroeconomic Data

Furthermore, we combine the data with macroeconomic variables. Primarily, we use the 3-month Overnight Index Swap (OIS) rate on the date of loan issuance to measure the monetary policy stance, which we denote as *Monetary policy*. The OIS rate has the advantage that it varies on a daily basis and allows us to exploit within-quarter variation. If no observation is available because the date is not a business day, we use the last available observation before the date of inception. We use the OIS rate rather than the ECB policy rate(s) for two reasons. First, the ECB sets different policy rates for its main refinancing operations (MRO) and the deposit facility (DF). Second, in an environment of excess central bank liquidity, risk-free short-term interest rates will settle somewhere between the MRO and the DF rate. Hence, by taking the OIS rate as our monetary policy measure, we are using the de facto short-term risk-free interest rate that materializes as a result of monetary policy actions. Furthermore, we prefer the OIS rate rather than the EURIBOR rate, as the latter is an interbank rate and can, therefore, also include some risk premia.

In addition, we include the yield on the 10-year government bondand the monthly Country-Level Index of Financial Stress[8] as control variables.

In the second part of our empirical analysis, we study heterogeneity depending on the importance of the corporate bond market. We calculate *Importance bond market* as the share of outstanding amounts of debt securities by the non-financial firms of a country relative to the sum of this amount and the bank loan amount, such that a higher value indicates a higher importance of the corporate bond market. This data is only available starting 2021. We use the value in the preceding quarter.

Table 3.3.3 reports descriptive statistics for the macroeconomic variables at the loan level. Throughout most of our sample, monetary policy was at the zero lower bound, resulting in a slightly negative OIS rate. However, due to the tightening of the monetary policy recently, we cover a wide range of monetary policy from -0.6 to 3.1 percentage points. The importance of the corporate bond market also varies substantially. As we will show below, this variation is mainly across countries rather than over time.

8. See Duprey, Klaus, and Peltonen (2015) for details on this index.

Table 3.3.3. Summary statistics: Macroeconomic Data

	Mean	SD	Min	P25	Median	P75	Max	N
Monetary policy	−0.1	0.9	−0.6	−0.5	−0.5	−0.4	3.1	14374070
MP shocks	18.3	13.1	−1.3	7.4	20.4	24.3	52.8	14374070
Importance bond market	22.8	6.9	2.3	18.0	19.6	23.4	52.8	5661817
10-year Goverment Bond Yield	1.1	1.2	−0.8	0.0	0.8	1.9	5.4	14374070
Country-Level Index of Financial Stress	0.1	0.1	0.0	0.1	0.1	0.2	0.6	14374070

Notes: The table reports summary statistics for the macroeconomic data of new loans issued by euro area banks to non-financial firms between Q1 2019 and Q1 2023. *Monetary policy* is the 3-month OIS rate on the date of issuance. *MP shocks* are defined in equation (3.4.3) as the cumulative sum of high-frequency monetary policy shocks to the 3-month OIS rate, provided by Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019). *Importance bond market* is defined as the share of outstanding amounts of debt securities by the non-financial firms of a country relative to the sum of this amount and the bank loan amount. Details on the Country-Level Index of Financial Stress can be found in Duprey, Klaus, and Peltonen (2015).

3.4 Empirical Results

In this section, we first present our empirical strategy, which we use to empirically analyze the impact of monetary policy and macroprudential policy on lending rates. Next, we present the results. We demonstrate that monetary policy dominates relative to macroprudential policy. Then, we show that the dominance is lower at the zero lower bound, when the corporate bond market is less important, and when banks have more capital.

3.4.1 Empirical Strategy

We use a rich set of control variables and fixed effects to estimate the impact of monetary policy and macroprudential policy on lending rates determined by banks.

We want to isolate the impact of monetary policy and bank capitalization on banks' pricing of new loans. To do so, we eliminate other factors that have been shown to affect the interest rate of new loans, which may be correlated with monetary policy or bank capitalization as well as credit demand. We estimate the following loan-level regression specification:

$$\begin{aligned} \text{interest rate}_i = & \beta \text{Monetary policy}_t \\ & + \rho \text{Bank capital}_{b,q-1} \\ & + \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} \\ & + \zeta_{b,f} + \theta_{f,q} + \varepsilon_i \end{aligned} \tag{3.4.1}$$

where i identifies a new loan, q, m, and t index the quarter, month and day of issuance, b the issuing bank, f the borrowing firm, and c the country of the firm. Our main coefficients of interest rate are β and ρ. β corresponds to the parameter in theoretical framework equation (3.2.2), i.e. the impact of *Monetary policy* on bank lending rates. The coefficient ρ differs from the $\tilde{\rho}$ from our theoretical framework to account for the fact that in the theoretical framework, we imposed a pass-through of funding costs to lending rates of 1-for-1. Thus, ρ should be thought of as the equity premium in model equation (3.2.2) multiplied by the pass-through of higher funding costs to lending rates. The relationship between β and ρ estimates the relative importance of monetary and macroprudential policy. To isolate the impact on credit supply, we add control variables and fixed effects, which we will describe next.

First, we ensure that β indeed captures the effect of monetary policy on banks' lending decisions. Firm-quarter fixed effects $\theta_{f,q}$ capture unobserved heterogeneity at the firm-quarter level, in particular credit demand (Khwaja and Mian, 2008), which could be responsive to monetary policy. We therefore use only the variation of $\textit{Monetary policy}_t$ within a given quarter to estimate the impact. A higher PD should imply higher interest rates. As banks can vary in their estimation of the PD (Berg and Koziol, 2017), γ_1 controls for this effect. Within quarters, macroeconomic conditions in the firm's country might change, which could affect banks' loan pricing. Therefore, γ_2 controls for the effect of the 10-year government bond yield of the firm's country. This controls for country-specific macroeconomic risk. $X_{c,m}$ furthermore includes the CLIFS indicator as a measure of financial stress. The identification of β then rests on the assumption that firms do not systematically adjust their credit demand to monetary policy within a quarter and that monetary policy does not affect other macroeconomic conditions that simultaneously affect bank lending beyond the variables we control for.

Second, we ensure that ρ indeed captures the effect of capitalization on loan rates. Bank capital is not determined exogenously and can be correlated with other variables that affect banks' lending decisions. On the one hand, γ_3 controls for a set of lagged bank characteristics. Specifically, we control for size, the NPL ratio, profitability, and the provisioning ratio. On the other hand, we include bank-firm fixed effects $\zeta_{b,f}$. These absorb time-invariant variation at the bank-firm level. This also ensures that ρ is only estimated from within bank variation in a bank's capitalization. Identification of ρ then rests on the assumption that there are no bank characteristics that vary over time, are correlated with *Bank capital* and affect lending rates, other than the four variables we control for.

3.4.2 Monetary Policy, Bank Capitalization, and Loan Rates

In this section, we document that the impact of monetary policy on lending rates dominates.

We start our analysis by examining the impact of monetary policy and bank capitalization on bank lending rates for non-financial firms and estimate equation (3.4.1) in our sample of new loans. Table 3.4.1 shows the regression results. We add the control variables and fixed effects in a stepwise procedure.

Table 3.4.1. Impact of Monetary Policy and Bank Capitalization on Lending Rates

Dependent variable:	Interest rate				
	(1)	(2)	(3)	(4)	(5)
Monetary policy$_t$	63.6***	63.5***	64.3***	64.7***	64.7***
	(4.5)	(4.5)	(4.5)	(4.5)	(4.5)
Bank capital$_{b,q-1}$	3.3***	1.8***	11.5***	13.0***	12.4***
	(0.61)	(0.60)	(2.4)	(2.7)	(2.9)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	No
Bank-Firm FE	No	No	No	Yes	Yes
Bank controls	No	Yes	No	No	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.80	0.80	0.81	0.86	0.86
R^2	0.82	0.82	0.83	0.87	0.88
Observations	14,374,070	14,323,950	14,374,055	14,234,966	14,185,197
p-value $\beta/\rho = 3$	0.00	0.01	0.04	0.08	0.09

Notes: This table shows empirical estimates of equation (3.4.1) for new loans issued on day t in quarter q by bank b with various sets of bank fixed effects and bank controls. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}_{b,q-1}$. Standard errors are clustered at the firm-bank and issuance day level.

Throughout specifications, we find - as expected - a strong positive relationship between *Monetary policy* rates and loan rates, i.e., positive and significant estimates for β. The estimate of the monetary policy pass-through is relatively stable across specifications and varies between 63.5 bps and 64.7 bps for a 100 bps change in monetary policy rates. The stable coefficient is not surprising as the specifications vary mainly in how we address heterogeneity at the bank level. Our estimates imply that banks transmit tighter monetary policy to lending rates of new corporate loans by around two-thirds. Our results for the impact of monetary policy on bank lending rates in the euro area during the most recent tightening cycle are in line with typical estimates found in the literature. In particular, in their meta-analysis Gregor, Melecký, and Melecký (2021) find that the average interest rate pass-through based on over a thousand estimates reported in the literature is around 80, and the conditional average is 60 when controlling for research methodologies, publication characteristics, and country macro-financial and institutional factors.

We now turn to the results regarding the impact of *Bank capital* on bank loan pricing, i.e., the estimates for ρ. As outlined above, the estimated coefficient bank capital should represent two factors: (i) the difference in the cost of bank equity and the cost of bank debt, i.e., the equity premium, and (ii) to what extent this funding cost difference is passed on to lending rates of bank customers. We find a positive relationship between bank capitalization and lending rates with coefficients between 1.8 and 13.0 bps, which are statistically significant. Adding time-varying bank control variables in column (2) reduces the estimate relative to column (1), highlighting the importance of controlling for other bank characteristics. Estimates increase substantially once control for unobserved heterogeneity at the bank level with bank fixed effects in column (3). This shows that within banks, differences in bank capitalization have a much stronger impact on lending rates than differences in bank capitalization across banks. The most saturated specification in column (5) results in an estimate for ρ of 12.4. Under the assumption of perfect pass-through of funding costs to lending rates, this implies an equity premium of 12.4, which is in the middle of the range estimates by Altavilla, Bochmann, et al. (2021). Under the assumption of an 80% pass-through, our estimate would imply an equity premium of 15.5, which is still in the range provided by Altavilla, Bochmann, et al. (2021).

We now compare the relative impact of monetary and macroprudential policies on bank lending rates and show that monetary policy dominates. For this purpose, we focus on column (5) of Table 3.4.1, our most stringent specification. For comparison, we fix the smallest possible monetary policy change of 25bps. This is passed on to lending rates as $0.25 \cdot 64.7 = 16.2$ bps higher lending rates. Which change in macroprudential buffer requirements would be needed to get a similar effect? Two factors intermediate the effect of macroprudential buffer requirements on *Bank capital*: As documented by Couaillier (2021), a change in buffer requirements affects banks' capital targets not one-for-one. Instead, he estimates a pass-through of 0.73[9]. We call this the *Capital target pass-through*. In addition, buffer requirements typically affect the risk-weighted capital ratio. For simplicity, we assume an average risk weight *RW* for loans to non-financial firms of 75%.[10] Together with our estimate for $\rho = 12.4$, this implies that to get the same effect as a 25bps monetary policy change, one would need a change in macroprudential buffer requirements of 2.4 pp. We derive this as follows:

9. See column (7) of Table 1 in Couaillier (2021).

10. For reference, the average risk weights for exposures to corporates in the euro area are around 85% and 45% under the standardized approach and the internal ratings-based approach, respectively.

Table 3.4.2. Impact of the Zero Lower Bound

Dependent variable:	Interest rate							
Monetary policy measure$_t$:	OIS rate				MP shocks			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Monetary policy measure$_t$	66.5***	66.4***	67.6***	67.6***	1.1***	1.1***	1.1***	1.1***
	(4.8)	(4.8)	(4.8)	(4.8)	(0.2)	(0.2)	(0.2)	(0.2)
Bank capital$_{b,q-1}$	-0.4	-1.2	10.6***	10.0***	-0.4	-1.2	10.6***	9.9***
	(0.8)	(0.8)	(2.7)	(2.9)	(0.8)	(0.8)	(2.7)	(2.9)
Monetary policy measure$_t$ × ZLB$_t$	-39.6*	-39.7*	-36.7	-36.6	-0.9**	-0.8**	-1.0***	-1.0***
	(22.2)	(22.2)	(22.6)	(22.7)	(0.4)	(0.4)	(0.4)	(0.4)
Bank capital$_{b,q-1}$ × ZLB$_t$	4.6***	4.0***	2.9***	2.8***	4.6***	3.9***	2.9***	2.8***
	(0.8)	(0.8)	(0.7)	(0.7)	(0.8)	(0.8)	(0.7)	(0.7)
ZLB$_t$	-30.4***	-25.2***	-16.1**	-15.8**	-28.6**	-23.4**	-12.2	-11.8
	(7.5)	(7.4)	(6.9)	(6.9)	(11.4)	(11.4)	(10.5)	(10.5)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	No	No	Yes	Yes	No	No	Yes	Yes
Bank controls	No	Yes	No	Yes	No	Yes	No	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.80	0.80	0.86	0.86	0.80	0.80	0.85	0.85
R^2	0.82	0.82	0.87	0.88	0.82	0.82	0.87	0.87
Observations	14,374,070	14,323,950	14,234,966	14,185,197	14,374,070	14,323,950	14,234,966	14,185,197

Notes: Columns (1) to (4) of this table show empirical estimates of equation (3.4.2) for new loans issued on day t in quarter q by bank b with various sets of bank fixed effects and bank controls. Columns (5) to (8) show empirical estimates of equation (3.4.4). The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. The *Monetary policy measure*$_t$ in columns (1) to (4) is the 3-month OIS rate on the date of issuance. In the remaining columns, it is the cumulative sum of high-frequency monetary policy shocks to the 3-month OIS rate, provided by Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa (2019), as defined in equation (3.4.3). *Bank capital*$_{b,q-1}$ is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}\quad b,q-1}$. *ZLB*$_t$ is a dummy variable that is 1 if and only if *Monetary Policy*$_t < 0$. Standard errors are clustered at the firm-bank and issuance day level.

$$
\begin{aligned}
\hat{\beta} \cdot \Delta\text{Monetary policy} &= \hat{\rho} \cdot \Delta\text{Bank capital} \\
&= \hat{\rho} \cdot \Delta\frac{\text{Capital}}{\text{Assets}} \\
&= \hat{\rho} \cdot \Delta\frac{\text{Capital}}{\text{RWA/RW}} \\
&= \hat{\rho} \cdot \text{RW} \cdot \Delta\frac{\text{Capital}}{\text{RWA}} \\
&= \hat{\rho} \cdot \text{RW} \cdot \Delta\text{Macroprudential buffer} \cdot \text{Capital target pass-through} \\
\iff \quad \Delta\text{Macroprudential buffer} &= \frac{\hat{\beta}}{\hat{\rho} \cdot \text{RW} \cdot \text{Capital target pass-through}} \cdot \Delta\text{Monetary policy} \\
&= \frac{64.7}{12.4 \cdot 75\% \cdot 0.73} \cdot 0.25 \; = \; 2.4
\end{aligned}
$$

3.4.3 The Zero Lower Bound

In this section, we explore heterogeneity along the time dimension and show that the relative importance of monetary compared to macroprudential policy decreases at the zero lower bound.

Our specification above did not allow the impact of monetary policy to vary with the macro-financial environment, particularly with whether monetary policy is at the zero lower bound. In this section, we extend our baseline specification to

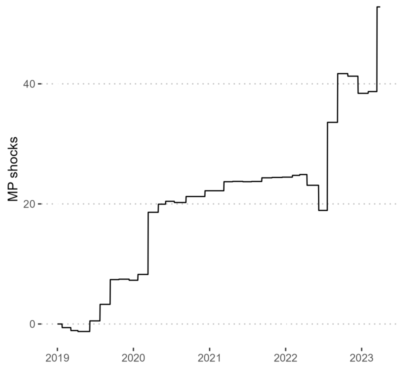


Figure 3.4.1. Monetary Policy Shocks Over Time

Notes: This figure plots the cumulative sum of monetary policy shocks obtained from (Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa, 2019), as defined in equation (3.4.3).

capture potential non-linearities concerning the *level* of policy rates. In order to do so, we estimate the following regression, in which we extend equation (3.4.1) with interaction terms:

$$\begin{aligned} \text{interest rate}_i = & \beta \text{Monetary policy}_t \\ & + \rho \text{Bank capital}_{b,q-1} \\ & + \beta_{ZLB} \text{Monetary policy}_t \times \text{ZLB}_t \\ & + \rho_{ZLB} \text{Bank capital}_{b,q-1} \times \text{ZLB}_t \\ & + \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} + \gamma_4 \text{ZLB}_t \\ & + \zeta_{b,f} + \theta_{f,q} + \varepsilon_i \end{aligned} \tag{3.4.2}$$

where ZLB_t is an indicator that is 1 if and only if the 3-month OIS rate, our measure of *Monetary policy*, is below 0.

Columns (1) to (4) of Table 3.4.2 show the empirical results, again with different ways to control for bank heterogeneity. Throughout specifications, we find a weaker pass-through of monetary policy at the zero lower bound, i.e., $\beta_{ZLB} < 0$. Therefore, the impact of monetary policy weakens at the zero lower bound. The estimate is not precisely estimated in columns (3) and (4). We address this below. In addition, we find that the impact of bank capital increases at the zero lower bound, as evident from the positive and significant estimates for ρ_{ZLB}. Overall, this shows that in a macro-financial environment where monetary policy is constrained at the zero lower bound, the importance of macroprudential policy relative to monetary policy increases. While away from the zero lower bound, the relation between *Monetary policy* and *Bank capital* is only slightly larger than the average effects estimated in Table 3.4.1 (β/ρ = 6.8 compared to 5.2), at the zero lower bound, the effect is about half (β/ρ =2.4).

The OIS rate does not vary much at the zero lower bound, as shown in Figure 3.1.1b in the introduction. Therefore, we additionally use monetary policy shocks to support our qualitative results at the zero lower bound, as there is more variation in monetary policy shocks. We use shocks to the 3-month OIS rate provided by Altavilla, Brugnolini, et al. (2019), which are identified from high-frequency market reactions around the short window of ECB monetary policy decisions. Since our variable of interest is the monetary policy rate in *levels*, we use the cumulative sum of monetary policy shocks from the beginning of our sample to day t to construct a time series:

$$\text{MP shocks}_t = \sum_{\tau=01.01.2019}^{t} \text{MP shock}_\tau \tag{3.4.3}$$

where t, τ describe business days. Figure 3.4.1 shows the substantial variation in this measure. We then replace *Monetary policy*$_t$ with *MP shocks*$_t$and estimate the following variation of equation (3.4.2):

$$\begin{aligned} \text{interest rate}_i = & \beta^{\text{shocks}} \text{MP shocks}_t \\ & + \rho^{\text{shocks}} \text{Bank capital}_{b,q-1} \\ & + \beta_{ZLB}^{\text{shocks}} \text{MP shocks}_t \times \text{ZLB}_t \\ & + \rho_{ZLB}^{\text{shocks}} \text{Bank capital}_{b,q-1} \times \text{ZLB}_t \\ & + \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} + \gamma_4 \text{ZLB}_t \\ & + \zeta_{b,f} + \theta_{f,q} + \varepsilon_i \end{aligned} \tag{3.4.4}$$

We show the results in columns (5) to (8) of Table 3.4.2 . The estimate of 1.1 for β^{shocks} shows that the cumulative sum of monetary policy shocks is transmitted almost one-for-one to loan rates. However, consistent with our results above, the negative significant estimates for $\beta_{ZLB}^{\text{shocks}}$ show that the pass-through is significantly smaller. In these estimates, pass-through is even close to zero when the 3-month OIS rate is negative.

3.4.4 The Importance of the Corporate Bond Market

Next, show that lower importance of the corporate bond market in a country reduces the dominance of monetary policy.

Figure 3.4.2 shows the variation in the importance of the corporate bond market, defined as the share of the outstanding amount of corporate bonds to non-financial firms relative to the sum of outstanding loans and bonds. Each dot represents a country-quarter observation. The figure shows that the heterogeneity in the importance of the bond market is mainly across rather than within countries. A larger corporate bond market implies stronger competition for banks with respect to providing funds to non-financial firms (Holm-Hadulla and Thürwächter, 2021). This

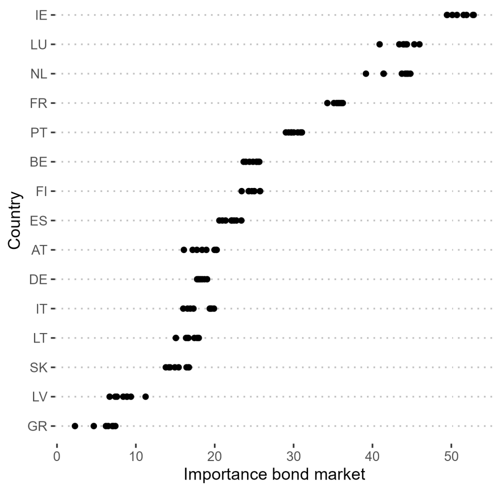


Figure 3.4.2. Importance of the Corporate Bond Market Across Countries

Notes: This figure shows the importance of the corporate bond market in financing non-financial firms. Each dot represents a country-quarter observation. *Importance bond market* is defined as the share of outstanding amounts of corporate bonds to non-financial firms relative to the sum of outstanding loans and bonds.

stronger competition could imply that it is more difficult for banks to pass on their funding costs to loan rates. However, an empirical question is whether this effect is stronger for monetary policy or bank capitalization. To study this, we estimate the following regression equation:

$$\begin{aligned}\text{interest rate}_i = & \beta \text{Monetary policy}_t \\ & + \rho \text{Bank capital}_{b,q-1} \\ & + \beta_{bonds} \text{Monetary policy}_t \times \text{Importance bond market}_{c,q-1} \\ & + \rho_{bonds} \text{Bank capital}_{b,q-1} \times \text{Importance bond market}_{c,q-1} \\ & + \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} \\ & + \zeta_{b,f} + \theta_{f,q} + \varepsilon_i\end{aligned} \tag{3.4.5}$$

Table 3.4.3 shows the empirical results. Due to the availability of *Importance bond market*, the number of observations drops. As hypothesized, we find negative and significant estimates for β_{bonds} throughout specifications, implying a weaker pass-through of monetary policy to loan rates when the corporate bond market is more important and intensifies competition. Similarly, we also find negative estimates for ρ_{bonds}, showing that banks can pass on their costs of capital less to loan rates when exposed to stronger competition from the corporate bond market. A one standard deviation (6.9pp) higher *Importance bond market* reduces the monetary policy pass-

Table 3.4.3. Impact of the Corporate Bond Market

Dependent variable:	Interest rate			
	(1)	(2)	(3)	(4)
Monetary policy$_t$	84.4***	84.7***	87.1***	87.1***
	(7.5)	(7.5)	(7.5)	(7.5)
Bank capital$_{b,q-1}$	12.6***	9.1***	21.0***	16.4***
	(2.1)	(1.9)	(5.0)	(4.6)
Monetary policy$_t$ $\times$ Importance bond market$_{c,q-1}$	-1.2***	-1.2***	-1.2***	-1.2***
	(0.2)	(0.2)	(0.2)	(0.2)
Bank capital$_{b,q-1}$ $\times$ Importance bond market$_{c,q-1}$	-0.4***	-0.4***	-0.9***	-0.9***
	(0.1)	(0.1)	(0.2)	(0.2)
Firm-Quarter FE	Yes	Yes	Yes	Yes
Bank-Firm FE	No	No	Yes	Yes
Bank controls	No	Yes	No	Yes
Firm controls	Yes	Yes	Yes	Yes
Adjusted R^2	0.84	0.84	0.89	0.89
R^2	0.86	0.86	0.91	0.91
Observations	5,661,817	5,661,169	5,582,457	5,581,864
Importance interaction for *Monetary policy*	1%	1%	1%	1%
Importance interaction for *Bank capital*	3%	5%	4%	5%

Notes: This table shows empirical estimates of equation (3.4.5) for new loans issued on day t in quarter q by bank b to a firm in country c with various sets of bank fixed effects and bank controls. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}_{b,q-1}$. *Importance bond market$_{c,q-1}$* is defined as the share of outstanding amounts of debt securities by the non-financial firms of a country relative to the sum of this amount and the bank loan amount. Standard errors are clustered at the firm-bank and issuance day level.

through by 9.5% and the impact of *Bank capital* by even 38%.[11] Consequently, with higher importance of the bond market, the dominance of monetary policy on bank lending rates relative to macroprudential policy increases. However, this does not necessarily imply that the overall impact on firms' funding cost, i.e., the costs for bank and bond financing, also increases, as this depends on the dynamics of the corporate bond market.

3.4.5 Bank capitalization

Finally, we document the impact of the level of bank capitalization on the relative importance of monetary and macroprudential policy.

To do so, we proceed in two steps. We begin by studying the impact of the level of bank capital. Imbierowicz, Löffler, and Vogel (2021) show that higher capital requirements can attenuate the transmission of accommodative monetary policy to lending rates while Altavilla, Laeven, and Peydró (2020) provide evidence for complementary effects on loan quantities but do not analyze interest rates. To study the effect of the level of bank capital, we interact both of our variables of interest with *Bank capital* and estimate the following regression equation:

$$\begin{aligned} \text{interest rate}_i \ =&\beta \text{Monetary policy}_t \\ &+ \rho \text{Bank capital}_{b,q-1} \\ &+ \beta_{capital}\text{Monetary policy}_t \times \text{Bank capital}_{b,q-1} \\ &+ \rho_{capital}\text{Bank capital}^2_{b,q-1} \\ &+ \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} \\ &+ \zeta_{b,f} + \theta_{f,q} + \varepsilon_i \end{aligned} \tag{3.4.6}$$

$\beta_{capital}$ and $\rho_{capital}$ estimate how the impact of monetary policy and bank capitalization depends on the level of banks' capital. Columns (1) to (4) of Table 3.4.4 show the empirical results for estimating this equation. We find negative and significant estimates for $\beta_{capital}$ throughout specifications. This implies that the pass through of monetary policy to lending rates is weaker for firms with more capital. Possibly, their higher capital levels allow them to cut lending supply less when monetary tightens. We also find weak evidence for a non-linear relationship between lending rates and *Bank capital*, as the coefficient $\rho_{capital}$ on the squared variable is negative. However, the coefficient is no longer significant once we add bank-firm fixed effects in column (3). Again, the last two rows in this table compare the importance of the interaction for both variables of interest. The dampening effect of the level of bank capital on the transmission of monetary policy is substantially larger than the effect

11. $1.2 \cdot 6.9/87.1 = 9.5\%$ and $0.9 \cdot 6.9/16.4 = 38\%$.

Table 3.4.4. Impact of the Level of Bank Capital

Dependent variable:	Interest rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Monetary policy$_t$	82.6***	80.8***	83.0***	82.9***	98.2***	101.1***
	(5.6)	(5.6)	(5.8)	(5.8)	(8.5)	(8.3)
Bank capital$_{b,q-1}$	6.3***	4.2***	15.4***	14.3***		
	(1.3)	(1.2)	(2.8)	(2.9)		
Monetary policy$_t$ × Bank capital$_{b,q-1}$	-2.5***	-2.2***	-2.4***	-2.4***	-4.3***	-2.2**
	(0.4)	(0.4)	(0.4)	(0.4)	(0.8)	(1.1)
Bank capital$_{b,q-1}{}^2$	-0.1***	-0.1***	-0.1	-0.1		
	(0.0)	(0.0)	(0.1)	(0.1)		
Monetary policy$_t$ × Capital headroom$_{b,t-1}$						-5.7***
						(1.5)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	No	No	Yes	Yes	Yes	Yes
Bank-Quarter FE	No	No	No	No	Yes	Yes
Adjusted R^2	0.80	0.80	0.86	0.86	0.86	0.86
R^2	0.82	0.82	0.87	0.88	0.88	0.88
Observations	14,374,070	14,323,950	14,234,966	14,185,197	14,234,505	14,234,131
Importance interaction for *Monetary policy*	3%	3%	3%	3%	4%	2%
Importance interaction for *Bank capital*	2%	2%	1%	1%		

Notes: Columns (1) to (4) of this table show empirical estimates of equation (3.4.6) for new loans issued on day t in quarter q by bank b with various sets of bank fixed effects and bank controls. Column (5) shows a variant that additionally includes bank-quarter fixed effects. Column (6) includes a further variant with an additional interaction term. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}{}_{b,q-1}$. *Capital headroom$_{b,q-1}$* is the distance between the actual capital ratio and the regulatory requirement in percentage points. Standard errors are clustered at the firm-bank and issuance day level.

on bank capital. This implies that when banks have a higher level of capital, the impact of monetary policy relative to macroprudential policy decreases. In particular, a one standard deviation higher *Bank capital* (2.5pp) reduces the pass-through of *Monetary policy* by 7.2% and the impact of *Bank capital* by only 1.7%.[12]

Since $\beta_{capital}$ is estimated from variation at the day-bank level, we can include bank-quarter fixed effects. Column (5) shows the results. The magnitude of the estimate for $\beta_{capital}$ almost doubles compared to column (4), showing the importance of the level of bank capital for the transmission of monetary policy. We get back to the results in column (6) below.

Bank capital can be low for different reasons. On the one hand, it could be lower because banks have to fulfill lower capital requirements, e.g., due to lower risk weights, lower buffer requirements for systemically important banks, or lower capital requirements under the Pillar 2 requirements. On the other hand, it could be lower because banks accept a smaller distance between their regulatory requirements and their actual level of capital, the so-called *Capital headroom*. When this capital headroom is smaller than desired, it can impact loan pricing. As it is difficult for banks to raise new equity quickly in the short run, banks can end up in situations where they are equity-constrained, i.e., where they cannot jointly attain their desired bank capitalization and loan volume. In such situations, banks could reduce lending to increase their capitalization for a given level of available equity. This reduction in loan supply for equity-constrained banks should then result in higher interest rates as long as loan demand does not drop to the same extent. We include interaction terms with the banks' capital headroom to capture such state dependence. The capital headroom captures the distance between the banks' capital ratio and the banks' regulatory capital requirement, and the closer it is to zero, the more likely it is that the bank could be equity-constrained. Thus, we estimate the following regression equation:

$$\begin{aligned}\text{interest rate}_i = &\beta \text{Monetary policy}_t \\ &+ \rho \text{Bank capital}_{b,q-1} \\ &+ \beta_{capitalheadroom} \text{Monetary policy}_t \times \text{Capital headroom}_{b,q-1} \\ &+ \rho_{capitalheadroom} \text{Capital headroom}_{b,q-1} \\ &+ \gamma_1 PD_{b,f,m} + \gamma_2 X_{c,m} + \gamma_3 X_{b,q-1} \\ &+ \zeta_{b,f} + \theta_{f,q} + \varepsilon_i\end{aligned} \tag{3.4.7}$$

Table 3.4.5 shows the results. In columns (1) to (2), where we only include the interaction with bank capital, we find negative and significant estimates for $\rho_{capitalheadroom}$, implying that indeed the effect of *Bank capital* on lending rates is larger (smaller) for banks with less (more) *Capital headroom*. However, this effect

12. $2.5 \cdot 2.4/82.9 = 7.2\%$ and $2.5 \cdot 0.1/14.3 = 1.7\%$.

Table 3.4.5. Impact of Capital Headroom

Dependent variable:	Interest rate							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Monetary policy$_t$	63.6***	63.5***	64.7***	64.7***	80.9***	79.8***	74.2***	74.3***
	(4.5)	(4.5)	(4.5)	(4.5)	(4.6)	(4.6)	(4.7)	(4.7)
Bank capital$_{b,q-1}$	7.1***	5.5***	16.4***	14.9***	7.4***	6.1***	16.6***	15.2***
	(1.6)	(1.2)	(2.4)	(2.5)	(1.6)	(1.2)	(2.4)	(2.5)
Bank capital$_{b,q-1}$ × Capital headroom$_{b,t-1}$	-0.9***	-0.6***	-0.3	0.0	-0.9***	-0.7***	-0.4*	-0.1
	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)	(0.2)
Monetary policy$_t$ × Capital headroom$_{b,t-1}$					-5.2***	-4.9***	-2.9***	-2.9***
					(0.5)	(0.5)	(0.5)	(0.5)
Capital headroom$_{b,t-1}$	6.8***	4.1***	0.6	-2.8	6.3***	3.7***	0.7	-2.4
	(1.5)	(1.4)	(1.9)	(2.2)	(1.5)	(1.4)	(1.9)	(2.1)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	No	No	Yes	Yes	No	No	Yes	Yes
Bank controls	No	Yes	No	Yes	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.80	0.80	0.86	0.86	0.80	0.80	0.86	0.86
R^2	0.82	0.82	0.87	0.88	0.82	0.82	0.87	0.88
Observations	14,373,674	14,323,554	14,234,591	14,184,823	14,373,674	14,323,554	14,234,591	14,184,823
Importance interaction for *Monetary policy*					6%	6%	4%	4%
Importance interaction for *Bank capital*	12%	12%	2%	0%	12%	11%	2%	0%

Notes: Columns (1) to (4) of this table show empirical estimates of equation (3.4.7) for new loans issued on day t in quarter q by bank b with various sets of bank fixed effects and bank controls but without the interaction of *Monetary policy* and *Capital headroom*. Columns (5) to (8) show estimates for the full equation. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}_{b,q-1}$. *Capital headroom$_{b,q-1}$* is the distance between the actual capital ratio and the regulatory requirement in percentage points. Standard errors are clustered at the firm-bank and issuance day level.

mainly holds across banks, as the estimates with bank-firm fixed effects in columns (3) and (4) are not significant.

Above, we have shown that the level of *Bank capital* also affects the pass-through of monetary policy to lending rates. Consequently, in columns (5) to (8), we also interact *Capital headroom* with *Monetary policy*. The estimates for $\beta_{capitalheadroom}$ are negative and significant, implying that a larger capital distance to regulatory requirements also leads to a weaker transmission of monetary policy to lending rates. This could be because firms with more *Capital headroom* have more capacity to maintain lending even when monetary policy tightens. As above, the last two rows or the table indicate the importance of the interaction effect. The estimates in columns (5) and (6) confirm the results above: At higher levels of bank capital headroom, the impact of monetary policy relative to macroprudential policy decreases. However, columns (7) and (8) show that this effect only holds across but not within banks.

We can now return to column (6) of Table 3.4.4, which studies variation within bank-quarter observations. It reveals that the transmission of monetary policy to lending rates is weaker for both higher levels of capital and larger capital headroom.

Combining the results on the interactions with *Bank capital* and with *Capital headroom*, we find that higher levels of both reduce the impact of monetary policy and bank capital on lending rates. The effects lead to a lower dominance of monetary relative to macroprudential policy for higher levels of capital throughout specifications and higher capital headroom across banks.

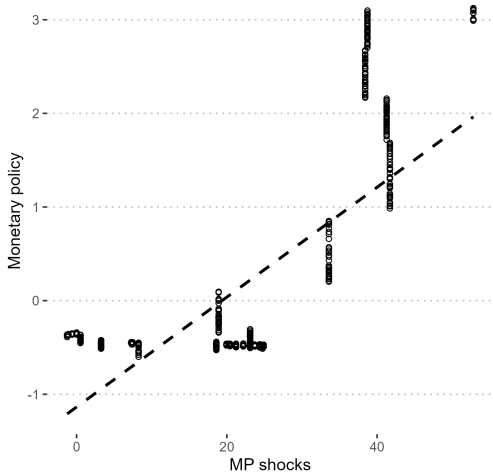


Figure 3.5.1. IV First Stage: Monetary Policy Shocks and OIS Rates

Notes: This figure presents visual evidence for the first stage of the IV regression described in equation (3.5.1). On the x-axis is the cumulative sum of monetary policy shocks provided by (Altavilla, Brugnolini, Gürkaynak, Motto, and Ragusa, 2019) as defined in equation (3.4.3). The y-axis shows the level of the 3-month OIS rate. Each dot represents a day on which at least one new loan was issued in our sample. The dashed line depicts the best linear fit.

3.5 Robustness

This section introduces two exercises that present robustness for our results. First, we employ IV estimations to isolate the exogenous component of monetary policy. Second, we explore the role of loan terms.

3.5.1 IV Estimations

We start by showing that our results are robust when using monetary policy shocks as an instrument for the level of monetary policy.

A concern about our results could be that monetary policy is not exogenous to economic developments even within a quarter. To address this, we employ an IV estimation strategy in this section. We use the cumulative sum of monetary policy shocks, *MP shocks*, defined above in equation (3.4.3) as an instrument for *Monetary policy*. Using the cumulative sum is similar to the approach in Bu, Rogers, and Wu (2021), Döttling and Ratnovski (2023), and Elliott, Meisenzahl, and Peydró (2024). Figure 3.5.1 plots for every day in our sample on which a loan was issued the cumulative sum of monetary policy shocks on the x-axis and the 3-month OIS rate on the y-axis. It shows that, as in the aforementioned papers, the cumulative sum is a relevant instrument for the level of monetary policy. We then estimate the following IV regressions:

Table 3.5.1. IV estimates

Dependent variable:	Interest rate				
	(1)	(2)	(3)	(4)	(5)
Monetary policy$_t$	47.1***	47.0***	47.9***	47.3***	47.3***
	(6.6)	(6.6)	(6.6)	(6.6)	(6.6)
Bank capital$_{b,q-1}$	3.3***	1.8***	11.4***	13.0***	12.3***
	(0.61)	(0.60)	(2.4)	(2.7)	(2.9)
Firm-Quarter FE	Yes	Yes	Yes	Yes	Yes
Bank FE	No	No	Yes	No	No
Bank-Firm FE	No	No	No	Yes	Yes
Bank controls	No	Yes	No	No	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.80	0.80	0.81	0.86	0.86
R^2	0.82	0.82	0.83	0.87	0.88
Observations	14,374,070	14,323,950	14,374,055	14,234,966	14,185,197

Notes: This table shows empirical IV estimates of equation (3.5.1) for new loans issued on day t in quarter q by bank b with various sets of bank fixed effects and bank controls. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}_{b,q-1}$. Standard errors are clustered at the firm-bank and issuance day level.

$$\begin{aligned} \text{1st stage: Monetary policy}_t \;&=\; \nu_1 \text{MP shocks}_t \\ &+\; \nu_1 \text{Bank capital}_{b,q-1} \\ &+\; \nu_2 PD_{b,f,m} + \nu_3 X_{c,m} + \nu_4 X_{b,q-1} \\ &+\; \zeta_{b,f} + \theta_{f,q} + \varepsilon_i \end{aligned} \tag{3.5.1}$$

$$\begin{aligned} \text{2nd stage: interest rate}_i \;&= \beta^{IV} \widehat{\text{Monetary policy}}_t \\ &+\; \rho^{IV} \text{Bank capital}_{b,q-1} \\ &+\; \gamma_1^{IV} PD_{b,f,m} + \gamma_2^{IV} X_{c,m} + \gamma_3^{IV} X_{b,q-1} \\ &+\; \zeta_{b,f}^{IV} + \theta_{f,q}^{IV} + \varepsilon_i^{IV} \end{aligned} \tag{3.5.2}$$

We adjust the estimation relative to the OLS specification in equation (3.4.1) and remove the yield on the 10-year government bond from $X_{c,m}$ as this could otherwise also be highly predictive for *Monetary policy*.

Table 3.5.1 shows empirical estimates of equation (3.5.1). The estimates for ρ on the impact of *Bank capital* remain hardly unchanged. In contrast, the estimates for β^{IV} are around a quarter lower than in the OLS estimates in Table 3.4.1. The observed reduction in the pass-through coefficient suggests that OLS may overstate the true causal impact of monetary policy on bank lending rates. One explanation for this could be that banks adjust lending rates not only to the current level of monetary policy rates but also price in expected future changes. Throughout our sample period, monetary policy rates exhibited an auto-correlated behavior, which might result in upward-biased OLS estimates if expected by banks.

The IV estimation isolates the exogenous component of policy changes, thereby providing a more accurate measure of their causal effect. Revisiting our comparison of the relative importance of monetary and macroprudential policy, with the IV estimates, one would require a 1.8 increase in macroprudential buffer requirements to get the same effect on lending rates as a 25bps monetary policy tightening, compared to a 2.4 increase in macroprudential buffer requirements based on the OLS estimates. This would still amount to a very big change in buffer requirements to get the same effect as the smallest possible change in monetary policy rates.

3.5.2 Loan Terms

Finally, we provide evidence that the dominance of monetary relative to macroprudential policy is not due to differences in loan terms.

A second concern with our results above could be that other loan terms than the interest rate might explain the results. For example, loans with different maturities should have different interest rates, even when issued to the same firm at the same time. Thus, a concern could be that *Monetary policy* and *Bank capital* also affect the type of loans that are issued. Assume, for example, that banks with higher *Bank capital* only issue low maturity loans, then the actual effect of *Bank capital* on loan rates would be higher than in our estimations, as the empirical estimate captures both, the effect of high capital and low maturity. In order to address this, we re-estimate equation (3.4.1) and add loan terms that have been used in the literature as control variables (e.g. Jiménez, Ongena, Peydró, and Saurina, 2014; Ioannidou, Ongena, and Peydró, 2015; Berg, Saunders, Steffen, and Streitz, 2017; Dell'ariccia, Laeven, and Suarez, 2017; Schwert, 2020; Luck and Santos, 2023). In particular, we include the log loan amount, a dummy for whether the bank has recourse or not in case of default, a dummy for whether the loan is fixed or floating, a categorical variable for the amortization scheme, a categorical variable for the type of instrument (e.g., term loan, credit line), a dummy whether the loan is collateralized or not, and maturity bucket-year fixed effects. We do not use these loan terms as control variables in our main specifications as this specification is prone to the "bad controls" problem (Cinelli, Forney, and Pearl, 2024), since the loan terms themselves could be affected by *Bank capital* or *Monetary policy*.

We present the results with loan controls in Table 3.5.2. Compared to the results in Table 3.4.1 without loan controls, the estimates for β hardly change. The estimates for ρ change somewhat in columns (1) to (4), but in the most stringent specification in column (5), estimates are almost identical. This suggests that our first key result, the relative dominance of monetary policy on lending rates relative to macroprudential policy, is not driven by differences in loan terms.

Table 3.5.2. Estimation with Loan Controls

Dependent variable:			Interest rate		
	(1)	(2)	(3)	(4)	(5)
Monetary policy$_t$	64.1***	64.0***	64.7***	65.0***	65.0***
	(4.4)	(4.5)	(4.4)	(4.4)	(4.4)
Bank capital$_{b,q-1}$	2.8***	2.4***	12.5***	13.6***	12.3***
	(0.60)	(0.58)	(2.2)	(2.5)	(2.8)
Bank FE	No	No	Yes	No	No
Bank-Firm FE	No	No	No	Yes	Yes
Bank controls	No	Yes	No	No	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.81	0.81	0.82	0.86	0.86
R^2	0.83	0.83	0.83	0.88	0.88
Observations	14,374,070	14,323,950	14,374,055	14,234,966	14,185,197
p-value $\beta/\rho = 3$	0	0	0.03	0.07	0.07

Notes: This table shows empirical IV estimates of equation (3.4.1) for new loans issued on day *t* in quarter *q* by bank *b* with various sets of bank fixed effects and bank controls. In addition to the original equation, the estimations also include loan terms as control variables. The sample consists of new loans issued by euro area banks to non-financial firms between Q1 2019 and 2023Q1. The dependent variable *Interest rate* is the interest rate of a loan on the issuance day, measured in basis points. *Monetary policy$_t$* is the 3-month OIS rate on the date of issuance. *Bank capital$_{b,q-1}$* is defined as $100 \cdot \frac{\text{Capital}_{b,q-1}}{\text{Assets}}{}_{b,q-1}$. *Loan controls* include the log loan amount, a dummy for whether the bank has recourse or not in case of default, a dummy for whether the loan is fixed or floating, a categorical variable for the amortization scheme, a categorical variable for the type of instrument, a dummy for whether the loan is collateralized or not, and maturity bucket-year fixed effects. Standard errors are clustered at the firm-bank and issuance day level.

3.6 Conclusion

In this paper, we analyze the effect of monetary policy and bank capitalization on the interest rate on new loans to non-financial firms in the euro area in 2019-2023. Granular, confidential loan-level, and supervisory data allow us to control for loan demand and a set of control variables and firm characteristics. Consistent with our stylized model of bank funding costs, monetary policy has the predominant effect on loan rates relative to bank capitalization. However, we document economic conditions under which the dominance of monetary policy weakens. It is about halved at the zero lower bound and substantially reduced in financial systems where the corporate bond market is less important as well as for better-capitalized banks. Our result also holds when we employ IV estimation using high-frequency identified monetary policy shocks around policy decisions to provide robustness for the estimated pass-through coefficient of monetary policy and when we take into account the potential effects of loan terms. These findings highlight the importance of considering the financial environment when assessing the relative roles of monetary and macroprudential policy in shaping lending conditions.

As such, our paper provides avenues for further research. In particular, we study an episode of low and rising monetary policy rates. Further research could explore whether results are similar in times of monetary easing or whether asymmetric effects emerge, for instance, if bank capitalization plays a different role in accommodating policy rate cuts. Additionally, while our study focuses on the euro area, investigating whether these patterns hold in financial systems with different institutional frameworks, capital market structures, and macroprudential policy designs could provide further insights into the generalizability of our findings.

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